PHILOSOPHICAL TRANSACTIONS.

PART II.

PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXIV. For the Year 1784.

PART II.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

MDCCLXXXIV.

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PHILOSOPHICAL TRANSACTIONS.

XIX. *On the remarkable Appearances at the Polar Regions of the Planet* Mars, *the Inclination of its Axis, the Position of its Poles, and its ſpheroidical Figure; with a few Hints relating to its real Diameter and Atmoſphere.* *By* William Herſchel, *Eſq. F. R. S.*

Read March 11, 1784.

WHAT I have to offer on the ſubject of the remarkable appearances at the polar regions of Mars, as well as what relates to the inclination of the axis, the poſition of the poles, and the ſpheroidical figure of that planet, is founded on a ſeries of obſervations which I ſhall deliver in this paper; and

after they have been given in the order they were made, it will be eaſy to ſhew, by a few deductions from them, that my theory of this planet is ſupported by facts which will ſufficiently authoriſe the concluſions I have drawn from them. For the ſake of better order and perſpicuity, however, I ſhall treat each ſubject apart, and begin with the remarkable appearances about the polar regions. The obſervations on them were made with a view to the ſituation and inclination of the axis of Mars; for to determine theſe we cannot conveniently uſe the ſpots on its ſurface, in the manner which is practiſed on the ſun. The quantities to be meaſured are ſo ſmall, and the obſervations of the center of Mars ſo precarious, and attended with ſuch difficulties (ſince an error of only a few ſeconds would be fatal) that we muſt have recourſe to other methods.

When I found that the poles of Mars were diſtinguiſhed with remarkable luminous ſpots *, it occurred to me, that we might obtain a good theory for ſettling the inclination and nodes of that planet's axis, by meaſures taken of the ſituation of thoſe ſpots. But, not to proceed upon grounds that wanted confirmation, it became neceſſary to determine by obſervation, how far theſe polar ſpots might be depended upon as permanent; and in what latitude of the globe of Mars they were ſituated; for, if they ſhould either be changeable, or not be at the very poles, we might be led into great miſtakes by overlooking theſe circumſtances. The following obſervations will aſſiſt us in the inveſtigation of theſe preliminary points.

* A bright ſpot near the ſouthern pole, appearing like a polar zone, has alſo been obſerved by M. MARALDI. See Dr. SMITH's Optics, § 1094.

1777,

1777, April 17. 7 h. 50′. There are two remarkable bright ſpots on Mars. In fig. 1. tab. VI. they are marked *a* and *b*. The line AB expreſſes the direction of a parallel of declination. 10 feet reflector, 9 inches aperture, power 211 *.

10 h. 20′. They are both quite gone out of the diſk.

1779, This year, in all my obſervations on Mars, there is no mention of any bright ſpots, ſo that I believe there were none remarkable enough to attract my attention. However, as my view was particularly directed to the phænomena of this planet's diurnal rotation, it is poſſible I might overlook them.

1781, March 13. 17 h. 40′. 20 feet reflector. I ſaw a very lucid ſpot on the ſouthern limb of Mars of a conſiderable extent. See fig. 2.

June 25. 11 h. 36′. 7 feet reflector, power 227. Two luminous ſpots appeared at *a* and *b*, fig. 3.; *a* is larger than *b*.

12 h. 15′. With 460. *a* is thicker than *b*, but *b* is rather longer.

13 h. 12′. *a* is grown thicker, and *b* become thinner.

June 27. 11 h. 20′. The two lucid ſpots are on Mars.

June 28. 11 h. 15′. They are both viſible; *a*, fig. 4. is much thicker than *b*.

12 h. 55′. A line joining *a* and *b* does not go through the center.

June 30. 10 h. 48′. The ſpot *a* is viſible. fig. 5.

11 h. 35′. Both ſpots are to be ſeen.

* Phil. Tranſ. vol. LXXI. p. 127. and fig. 17.

1781, July 3. 10 h. 54′. *a* ſeems to be larger than I have ſeen it, fig. 6.

11 h. 24′. *b* is not yet viſible, fig. 7.

12 h. 36′. I perceive part of *b*, fig. 8.

July 4. 12 h. 9. *a* is very full; *b* extremely thin, and barely viſible.

12 h. 18′. *a* and *b* are not quite oppoſite each other.

12 h. 49′. *b* is increaſed.

July 15. 9 h. 54′. *a* is viſible, fig. 9.

11 h. 35′. *b* inviſible.

12 h. 12′. *b* not to be ſeen.

July 16. 11 h. 9′. The bright ſpot *a* is very large.

July 17. 11 h. 15′. No other bright ſpot but *a*.

July 19. 13 h. 31′. *a* viſible.

July 20. 10 h. 3′. I ſuppoſe the bright ſpot *a* on Mars is, very nearly, the ſouth pole; which therefore muſt lie in ſight. There is no ſecond bright ſpot *b* viſible to night.

10 h. 56′ *b* not viſible; the night very fine.

July 22. 11 h. 14′. At *a* and *b*, fig. 10. are bright ſpots; *a* is larger than *b*. Moſt probably the ſouth pole is in view, and the north pole juſt hid from our ſight. If the ſpots are polar, or nearly ſo, then *a* muſt, on a ſuppoſition of the ſouth pole's being in view, appear larger than *b*; and if *b* extend a little more from the north pole one way than another, it muſt be ſubject to ſome change in its appearance from the revolution of Mars on its axis.

July 30. 9 h. 43′. Both ſpots viſible.

Auguſt 8. 10 h. 4. Only *a* viſible, fig. 11.

Auguſt 17. 9 h. 21′. Only *a* in ſight.

1781,

1781, Auguſt 23. 8 h. 44′. *a* as uſual, and part of *b* viſible, fig. 12.

Sept. 7. The white ſpot *a* is very large.

1783, May 20. Mars has a ſingular appearance. At *a*, fig. 13. is the polar ſpot, which is bright, and ſeems to project above the diſk by its ſplendour, cauſing a break at *c*.

July 4. *a* is very bright.

July 23. 14 h. 45′. *a* is very lucid.

Auguſt 16. I ſaw the bright ſpot with the 20 feet reflector as uſual.

Aug. 26. The lucid ſpot on Mars is its ſouth pole, for it remains in the ſame place, while the dark equatorial ſpots perform their conſtant gyrations: it is nearly circular.

Aug. 29. The ſouth polar ſpot is in the ſame ſituation.

Sept. 9. As uſual.

Sept. 22. The ſouth polar ſpot is of a circular ſhape, and very brilliant and white. I had a beautiful and diſtinct view of it when it was about the meridian, and meaſured its little diameter in the equatorial direction of Mars. With a power of 932 it gave 1″ 41‴, and I ſaw it very diſtinctly. The outward edge of the ſpot came juſt up to the upper limb; a favourable hazineſs, taking off every troubleſome ray, gave me objects in general exceedingly well defined, eſpecially Mars.

Sept. 23. 9 h. 55′. The polar ſpot *a*, fig. 14. as uſual.

Sept. 24. The ſame.

1783, Sept. 25. 12 h. 30′. The bright ſouth polar ſpot *a*, fig. 15. ſeems to be fixed in its place, and goes nearly up to the margin of the diſk; it is perfectly round.

12 h. 55′. The track of the equatorial ſpots is incurvated, being convex towards the north, ſee *e*, *q*, fig. 23.: this confirms the white ſpot's being at the ſouth pole. With long attention I can perceive the edge of the diſk of Mars beyond the ſpot, extending about ¼ diameter of the ſpot.

Sept. 26. 12 h. 10′. The ſpot *a* is in a line with the center and the end of the hook, fig. 16.

Sept. 27, 28, 29. The ſpot as uſual.

Sept. 30. 10 h. 30′. The polar ſpot as in fig. 17.

Oct. 1. 9 h. 55′. I am inclined to think, that the white ſpot has ſome little revolution, and therefore is not with its center exactly at the pole of Mars; it is rather probable, that the real pole, though within the ſpot, may lie near the circumference of it, or one-third of its diameter from one of the ſides. A few days more will ſhew it, as I ſhall now fix my particular attention upon it.

10 h. 17′. The bright ſpot is certainly not ſo far upon the diſk as it uſed to be formerly, and is either reduced or has a ſmall motion; which of the two is the caſe will be ſeen in a few hours.

13 h. 3′. The bright ſpot has a little motion; for it is now come farther into the diſk.

I concluded now, in general, that none of the bright ſpots on Mars were exactly at the poles, though they could certainly not be far from them: for what has been juſt related of the

1ſt, 2d, and 3d of October 1783, ſhews plainly, that the appearance of the ſouthern ſpot *a* was a little affected by the diurnal motion of the planet; and the obſervations of the 3d and 4th of July 1781, ſhew alſo that the ſpot *b* could not be exactly at the north pole; and that, perhaps, the viſible branch of the latter extended pretty far towards the equator. However, the ſouth polar ſpot of the year 1783, being very ſmall and nearly round, afforded a good opportunity for determining its polar diſtance, by noting the different angles of poſition it aſſumed while Mars revolved on its axis; to this end many obſervations were taken at different hours of the ſame night, which will be found among the meaſures of the angles of poſition in the next diviſion of my ſubject. And ſince the different degrees of brilliancy, as well as the proportional apparent magnitude of the ſpot, would alſo contribute to the inveſtigation of this point, I continued my remarks on thoſe particulars, as follows.

1783, Oct. 2. 7 h. 59′. The bright ſpot near the ſouth pole is about half viſible.

Oct. 4. 8 h. 0′. The polar ſpot ſeems to project above the diſk as formerly, and is very ſmall.

Oct. 5. 11 h. 13′. The ſpot is very ſmall, and ſeems actually to be in the circumference.

11 h. 30′. The ſpot is ſmall, and ſeems to be with its fartheſt ſide in the circumference of the diſk; or it may, perhaps, be partly beyond it, and therefore not all in ſight.

11 h. 50′. I ſee the ſpot much clearer than I did before.

13 h. 15′. The white ſpot is more in ſight, and of its uſual ſize, but does not ſeem much to change its poſition;

sition; however, what change there is shews that it has been beyond the pole, as it appears to have been direct while the equatorial spots were retrograde.

1783, Oct. 9. 11 h. 48′. The white polar spot increases in size. At 10 h. 35′. it was as in fig. 18. but is now larger, and coming round towards that part of its orbit which is nearest to us. See fig. 24.

Oct. 10. 6 h. 20′. I see no white polar spot; but the planet is too low for any observation to be depended on.

6 h. 55′. The white spot begins to be visible; at least I see it now, the planet being higher than before, fig. 19.

9 h. 55′. With 460, the white spot is considerably increased, and shews a circular form, fig. 20.

Oct. 11. 7 h. 46′. The bright spot is very visible; the evening fine; with 278.

Oct. 16. 7 h. 7′. The spot is very luminous.

9 h. 55′. It seems rather lengthened; perhaps it may be arrived at the extreme of its parallel of declination.

Oct. 17. 7 h. 47′. The white spot *a*, fig. 21. is very bright.

13 h. 7′. It is less in appearance than it was in the beginning of the evening.

Oct. 23. 6 h. 46′. The bright spot is very large and luminous; I suppose it to be in the nearer parts of its little orbit.

7 h. 11′. It is situated as in fig. 22.

Oct. 24. 7 h. 1′. The white spot is very large.

Oct. 27. 8 h. 45′. It is very large and round.

Nov. 1. 7 h. 47′. The spot is round and bright.

1783, Nov. 11. The deficiency of light which occaſions Mars to appear gibbous, reaches over the ſouth polar ſpot towards the preceding limb, and hides it.

Nov. 14. Mars is gibbous, and the polar ſpot is thereby rendered invisible.

Nov. 17. 6 h. 0′. The ſouth polar ſpot is under the falcated defect of light.

6 h. 30′. I do not know whether there be not a faint glimpſe of the polar ſpot left; the weather is too bad to determine it.

I have added fig. 25. (tab. X.) to ſhew the connection of the 15th, 17th, 18th, 19th, 20th, 21ſt, and 22d figures, which complete the whole equatorial circle of appearances on Mars, as they were obſerved in immediate ſucceſſion. The center of the circle marked 17 is placed on the circumference of the inner circle, by making its diſtance from the center of the circle, marked 15, anſwer to the interval of time between the two obſervations, properly calculated and reduced to ſidereal meaſure. The ſame has been done with regard to the circles marked 18, 19, &c. And it will be found, by placing any one of theſe connected circles, ſo as to have its contents in a ſimilar ſituation with the figures in the ſingle repreſentation which bears the ſame number, that there is a ſufficient reſemblance between them; but ſome allowance muſt undoubtedly be made for the unavoidable diſtortions occaſioned by this kind of projection.

In order to bring theſe obſervations on the bright ſpots into one view, I have placed them at the circumference of three circles (ſee fig. 26, 27, 28. tab. VII. VIII. IX.) divided into degrees, repreſenting the parallels of declination in which they

revolved about the poles of Mars. The division of the circles marked 360 is where a spot passes that meridian of the planet which is turned towards the earth, and where, consequently, it appears to us in its greatest lustre. The motion of the spot is according to the numbers 30, 60, 90, and so on to 360. In calculating the daily places of the spots I have used the sidereal period of 24 h. 39′ 21″,67 determined in my paper on the rotation of Mars*; and have also made proper allowances for the alterations of the geocentric longitudes calculated from the situations of that planet given in the Nautical Almanack; by which means the sidereal is reduced to a proper synodical period.

The following three tables contain the result of the calculations, and serve to explain the arrangement of the observations in the circles. In the first column are the times when the observations were made. In the second, the sidereal places of the spot in degrees and minutes. In the third column are the geocentric longitudes of Mars at the time of the observations. In the fourth, the necessary corrections on account of these different longitudes. In the fifth column are the corrected or synodical places of the spots; and, according to the numbers in that column, they are marked on the circles, where consequently each spot is represented as it must have appeared to be situated at the time of observation.

* Phil. Trans. vol. LXXI. p. 134.

TABLE

TABLE I.

Time of observation.			Sider. place.		Geoc. longit.			Correction.		Synod. place.	
M.	D.	H. M.	D.	M.	S.	D.	M.	D.	M.	D	M.
June	25	11 36	359	31	9	24	35	+0	0	359	31
	25	12 15	0	0	9	24	35	+0	0	0	0
	25	13 12	13	52	9	24	34	−0	1	13	51
	27	11 20	357	28	9	24	12	−0	23	357	5
	28	11 15	316	40	9	24	1	+0	34	316	6
	30	10 48	290	56	9	23	38	+0	57	289	59
	30	11 35	302	23	9	23	38	+0	57	301	26
July	3	10 54	263	40	9	22	55	+1	40	262	0
	3	11 24	279	58	9	22	55	+1	40	260	18
	3	12 10	282	9	9	22	55	+1	40	280	29
	3	12 36	288	26	9	22	55	+1	41	286	48
	4	12 9	272	28	9	22	40	+1	55	270	25
	4	12 49	282	4	9	22	40	+1	55	280	9
	15	9 54	134	7	9	19	43	+4	52	139	15
	15	11 35	158	42	9	19	42	+4	53	153	49
	15	12 12	167	43	9	19	42	+4	53	162	49
	16	11 9	142	48	9	19	26	−5	9	137	39
	17	11 15	134	40	9	19	9	−5	26	129	14
	19	13 31	148	37	9	18	34	−6	1	142	36
	29	10 3	88	25	9	18	21	−6	14	82	11
	20	10 56	101	19	9	18	20	−6	15	95	4
	22	11 14	86	32	9	17	49	−6	46	79	46
	30	9 43	347	46	9	16	5	−8	30	339	16

TABLE II.

Time of observation.			Sider. place.		Geoc. longit.			Correction.		Syood. place.	
	D.	H.	M.	D.	M	S.	D.	M	D. M.	D.	M.
June	25	11	36	86	51	9	24	35	+1 40	88	31
	25	12	15	96	20	9	24	35	+1 40	98	0
	25	13	12	110	12	9	24	34	+1 39	111	51
	28	11	15	53	0	9	24	1	+1 6	54	6
	30	10	48	27	16	9	23	38	+0 43	27	59
	30	11	35	38	43	9	23	38	+0 43	39	26
July	3	10	54	0	0	9	22	55	+0 0	0	0
	4	12	9	8	40	9	22	40	−0 15	8	25
	15	9	54	230	27	9	19	43	−3 12	227	15
	15	10	12	234	50	9	19	43	−3 12	231	38
	15	11	35	255	2	9	19	42	−3 13	251	49
	15	12	12	264	2	9	19	42	−3 13	260	49
	16	11	9	339	8	9	19	26	−3 29	235	39
	19	13	31	244	57	9	18	34	−4 21	240	36
	20	10	3	184	45	9	18	21	−4 34	180	11
	20	10	56	197	39	9	18	20	−4 35	193	4
	30	9	43	84	6	9	16	5	−6 50	77	16

TABLE

TABLE III.

Time of observation.			Sider. place.		Geoc. longit.			Correction.		Synod. place.	
	D.	H.	M.	D.	M.	S.	D.	M.	D.	M.	D.	M.
Sept.	25	13	30	6	32	0	9	54	+6	44	13	16
Oct.	1	10	17	262	5	0	8	6	+4	56	267	1
	1	13	3	302	29	0	8	5	+4	55	307	24
	2	7	59	218	55	0	7	50	+4	40	223	25
	4	8	0	200	0	0	7	15	+4	5	204	5
	4	8	46	211	12	0	7	15	+4	5	215	17
	5	11	13	237	23	0	6	55	+3	45	241	8
	5	11	30	241	31	0	6	55	+3	45	245	16
	5	11	50	246	23	0	6	55	+3	45	250	8
	5	13	15	267	4	0	6	54	+3	44	270	48
	5	14	0	278	1	0	6	53	+3	43	281	44
	7	8	20	176	8	0	6	23	+3	13	179	21
	7	10	5	201	41	0	6	22	+3	12	204	53
	7	11	50	227	14	0	6	21	+3	11	230	25
	9	12	48	207	35	0	5	49	+2	39	210	14
	10	6	55	126	42	0	5	37	+2	27	129	9
	10	7	50	140	5	0	5	36	+2	26	142	31
	10	9	55	170	30	0	5	34	+2	24	172	54
	10	12	11	203	36	0	5	33	+2	21	205	57
	16	7	7	72	9	0	4	15	+1	5	73	14
	16	7	46	81	39	0	4	15	+1	4	82	43
	16	9	55	113	2	0	4	14	+1	4	114	6
	17	7	47	72	19	0	4	3	+0	53	73	12
	17	13	7	150	13	0	4	0	+0	50	151	1
	23	6	46	0	0	0	3	10	−0	0	0	0
	24	7	1	354	0	0	3	2	−0	8	353	52

From the appearance and disappearance of the bright north polar spot in the year 1781, we collect that the circle of its motion, represented by fig. 26. was at some considerable distance from the pole. By a calculation, made according to the principles hereafter explained, its latitude must have been about 76° or 77° north; for I find that, to the inhabitants of Mars, the declination of the sun, June 25. 12 h. 15′ of our time, was about 9° 56′ south *; and the spot must have been at least so

* See p. 259. and 260.

far

far removed from the north pole as to fall a few degrees within the enlightened part of the disk, to become visible to us.

The south pole of Mars could not be many degrees from the center of the large bright southern spot of the year 1781, whose course is traced in fig. 27; though the spot was of such a magnitude as to cover all the polar regions farther than the 70th or 65th degree, and in that part which was on the meridian July 3, at 10 h. 54', perhaps a little farther.

In the next division of our subject will be shewn, that the inclination and position of the axis of Mars are such, that the whole circle, fig. 28. (which will appear to be in about 81° 52' of south latitude on the globe of Mars) was in view all the time the observations on the bright south polar spot of the year 1783, which are marked upon it, were made, but in so oblique a situation as to be projected into a very narrow ellipsis. See fig. 24. where *mn* is the little ellipsis in which the spot *a* revolved about the pole. Hence then we may easily account for the observed magnitude and brightness of the spot Oct. 23, 24, and 27. when it was exposed to us in its meridian splendour. Its situations Oct. 16. and 17. on one extreme of the parallel, as well as those of Oct. 5. and Nov. 1. on the other, gave us also a bright view of it: and, when we pass over to that half of the circle which lies beyond the pole, the much greater obliquity into which the spot must there be projected will perfectly account for its being smaller at 13 h. 7' of Oct. 17. than at 7 h. 47' of the same evening. It will also explain its smallness Oct. 4. and its increase Oct. 9. We shall have occasion hereafter to recur to the same figure, so that I take no notice at present of the angles of position which are marked upon it.

2 *Of*

Of the direction or nodes of the axis of Mars, *its inclination to the ecliptic, and the angle of that planet's equator with its own orbit.*

From the foregoing article we may gather, that the bright polar ſpots on Mars are the moſt convenient objects for determining the ſituation of the axis of this planet; I ſhall therefore collect, in one view, all the meaſures I have taken of theſe ſpots for that purpoſe. Before I conſtructed a micrometer for taking the angle of poſition, I uſed to draw a line through the figure delineated of Mars to repreſent the parallel of declination; in a few of my firſt obſervations, therefore, I can only take the ſituation of the polar ſpots from ſuch drawings, and of conſequence no great accuracy in the angles, as to the exact number of degrees, can be expected.

1777, April 17. 7 h. 50′. A line drawn through the middle of the two bright polar ſpots *a* and *b*, fig. 1. makes an angle of about 63°, with a parallel of declination AB; the ſouthern ſpot preceding and the northern following.

My reaſon for chuſing a line drawn through both the ſpots rather than through one of them and the center is, firſt, that they were not ſituated quite oppoſite each other, and therefore, unleſs other obſervations had pointed out which was moſt polar, I ſhould evidently run the greater riſk in fixing on one of them in preference to the other. In the next place, we find by the ſecond obſervation, page 235. that in two hours and a half both ſpots were intirely gone out of the diſk. This

plainly denotes, that they were both in the ſame half of a ſphere orthographically projected, and divided by a plane paſſing through the axis of Mars and the eye, but that neither of them were polar. Now, a line drawn through two points not far from oppoſite each other, both in the ſame hemiſphere, and both removed from the poles of it, muſt approach more to a parallelism with the axis, than a line drawn through either of them and the center.

1779, May 9. There being no bright ſpots by which to judge of the poſition of the poles, it is eſtimated from a well known dark equatorial ſpot, with a line drawn through the figure to denote a parallel of declination. By very rough eſtimation it is about 42° ſouth preceding.

May 11. The ſame figure, being drawn again in another ſituation, and alſo with a line giving a parallel of declination, points out, by the ſame rough eſtimation, 62° ſouth preceding.

1781, June 25. 11 h. 35′. The poſition of the ſpots *a* and *b*, fig. 3. with regard to a parallel of declination, meaſured with a micrometer 74° 32′. The ſpot *a* was ſouth preceding, and *b* north following.

July 15. 10 h. 12′. The angle of poſition, of the center of the ſpot *a*, fig. 9. through the center of the diſk, 74° 18′ ſouth preceding.

1783, Auguſt 16. Poſition of the ſpot *a*, 64° ſouth following the center; but as the planet is not full, the center becomes dubious, and the meaſure therefore may not be quite accurate, though taken with a 20 feet reflector; power 200.

Sept.

1783, Sept. 9. Position of the supposed south pole of Mars 65° 12′ south following; 7 feet reflector; power 460.

Sept. 22. Position of the same 52° 9′ s. following; 460.

Sept. 25. 13 h. 30′. Position of the south polar spot 56° 27′. very accurately taken, by bisecting the disk of Mars through the bright spot, and supposing the planet now near enough the opposition to induce no material error. Hitherto I have taken it through a supposed center by endeavouring to allow a little for what I thought the deficiency in the disk; but not to-night.

Oct. 4. 8 h. 46′. Position of the spot 51° 21′; Mars too low and hazy to depend much on the measure with so high a power as 460.

Oct 5. The motion of the polar spot being now strongly suspected, or rather already known, I took the following measures, by way of discovering its quantity.

11 h. 50′. Position very exactly taken 50° 6′ s. following.

14 h. 0′. Position of the spot 49° 45′.

Oct. 7. 8 h. 20′. Position 55° 12′. In order to see how far this measure might be trusted to, I set 49° 36′ in the micrometer, which was evidently too small; next I took 51° 36′, which was also too small; after this, I took a new measure, and found 55° 24′, which appeared to me very exact. 10 h. 5′. The position now was 53°. 11 h. 50′. It measured 52° 12′. As there is nothing to distinguish the center, it is extremely difficult to please one's self in bringing the spot into a line with it.

1783, Oct. 10. 7 h. 50′. Position of the polar spot 57° 12′; with 460, very accurate. I tried a few parts less of the micrometer, but found the measure too little. I see pretty distinctly, but the air is tremulous.

9 h. 55′. Position 52° 42′; very distinct.

12 h. 11′. Position 46° 30′; I see not quite so well now as I could wish.

14 h. 1′. Position 44° 12′; but liable to great uncertainty, on account of tremulous air; it becomes more difficult to distinguish the center when the planet is not perfectly defined.

Oct. 16. 7 h. 7′. Position 63° 9′. By way of trial I set 59° 36′, which was too small; also 60° 24′ was too small; again, 61° 24′ was not large enough. Then, taking a fresh measure, I found it 62° 48′, which I thought right.

9 h. 55′. I took three measures, and thought the third, which was 65° 0′, the best of all, for I saw the planet and the spot remarkably well.

Oct. 27. 8 h. 45′. Position of the polar spot 59° 30′. I took three other measures, of which 60° 39′ appeared to me the best; it was taken with long attendance and many changes and trials of the wires in different positions; but the gibbosity of Mars is such, that measures of the situation of the spot are now no longer to be depended on.

These positions, I believe, will be sufficient for the purpose of settling the latitude of the polar spots, and thereby obtaining a correct measure of the situation of the real pole. I have referred those of the south polar spot of the year 1783 to the same circle which contains the observations that were made on

the apparent brightneſs and magnitude of that ſpot, that they may be compared together. (See fig. 28.) The agreement of the meaſures, and the phænomena attending the motion of the ſpot, are ſufficient to point out the meridian of the circle; for which, from a due conſideration of theſe circumſtances, I have fixed on the place where the ſpot was Oct. 10. 6 h. 46′.

Of the angles collected in fig. 28. we find 65° 0′ the largeſt, and 49° 45′ the ſmalleſt; but, on account of the different ſituation of the earth and Mars, the angle meaſured 7′ leſs Oct. 16. than it would have done had the planets remained in the places they were in Oct. 5. when the other meaſure was taken. This being added, we have 65° 7′. The difference between the two poſitions is 15° 22′. Now, the conſtruction of fig. 28. being admitted, we ſee that the angles were nearly taken at the oppoſite extremes of the circle in which the ſpot moved. However, by the 5th column of Tab. III. Oct. 5. we have the ſituation of the ſpot in the circle with reſpect to the meridian 281° 44′, and Oct. 16. 114° 6′: therefore the ſouth polar diſtance of the center of the ſpot is found, by taking half the ſum of the ſines of theſe angles to radius, as 7° 41′ (half of 15° 22′) to a fourth number, which is 8° 8′; and the latitude of the circle, in which the ſpot moved about the pole, therefore is 81° 52′ ſouth. This being determined, we have the following correction for the angles of poſition: radius is to ſine of the angular diſtance of the ſpot from the meridian as 8° 8′ to the required quantity. This muſt be added or ſubtracted, according as the caſe requires; and thereby we ſhall have the poſition of the true pole from any one of the meaſures.

I ſhall now apply the above to determine the ſituation of the axis of Mars. To this end, we ſee that, in the firſt place, the

meaſures muſt be corrected for the latitude of the ſpot; next, they muſt be reduced to a heliocentric obſervation, which will alſo correct them from the difference occaſioned by the different ſituation of the planets when they were taken. This being done, we may ſelect two obſervations at a proper diſtance; from which, by trigonometry, we ſhall have the node and inclination of the axis. When theſe elements are obtained, it will be eaſy to ſee how other obſervations agree with them; which will afford the means of correcting or verifying the former calculations.

Let T, fig. 29. (tab. X) be the earth; ♋ Q *q* ♈ the ecliptic as ſeen from T; P the point of the heavens towards which the north pole of the earth is directed; M the place of the orbit of Mars μm M, where an obſervation of the poles of that planet has been made, which is to be reduced to its heliocentric meaſure. And, firſt, ſuppoſe it to have been made at the time of the oppoſition of that planet. Then, the place M or Q in the ecliptic being given, we have the ſides Q ♋, ♋ P; whence the angle Q, of the right-angled triangle P ♋ Q, is found. This being added to, or taken from, the obſerved angle of poſition of the axis of Mars, according to circumſtances eaſily to be determined, reduces it to its heliocentric poſition. But if this obſervation was not made at the time of an oppoſition, but at ſome other place *m*, a ſecond correction is to be applied in the following manner.

Let the angle *q*, of the triangle P ♋ *q*, be found as before, and properly applied to the poſition of the axis of Mars now at *m*; then make the angle $m S \mu$, at the ſun S, equal to the angle S *m* T, and μ will be the heliocentric place, where the angle of poſition, when ſeen from S, will appear to be as it was found at *m*, after the application of the firſt correction:

for S μ being parallel to T m, and ſuppoſing the axis of Mars to preſerve its parallelism while it moves from m to μ, appearances of Mars at μ to an eye at S, muſt be the ſame as they are at m to an eye at T.

The following table contains the reſult of calculations relating to the angles of fig. 28. In the firſt column are the times when the obſervations were made. In the ſecond, the angles as they were taken. In the third column are the quantities of the angles Q, q, calculated from the geocentric longitudes contained in the third column of the third table. In the fourth column are the corrections for the ſituation of the ſpot in the circle of latitude obtained from the ſines of the angles in the fifth column of the third table. In the fifth are the corrections requiſite on account of the change of ſituation of the planets, during the interval between the ſeveral days on which the meaſures were taken; theſe are obtained from the third column of this table, and I have aſſumed the 4th of October, as being the obſervation neareſt the oppoſition, to which I have reduced the other meaſures. In the ſixth column are the angles of the ſecond, corrected by the quantities contained in the fourth and fifth columns, applied according to their ſigns.

TABLE

TABLE IV.

Time of obſervation.			Angles taken,		Angle Q.		First correction.		Second correct.	Angles corrected.	
	D.	H.	M.	D.	M.	D.	M.	D.	M.	M.	D.	M.
Sept.	25	13	30	56	27	+23	10	−1	52	−8	54	27
Oct.	4	8	46	51	21	+23	18	+4	42	−0	56	3
	5	11	50	50	6	+23	19	+7	39	+1	57	46
	5	14	0	49	45	+23	19	+7	59	+1	57	45
	7	8	20	55	12	+23	21	−0	7	+2	55	7
				55	24						55	19
	7	10	5	53	0	+23	21	+3	26	+3	56	29
	7	11	50	52	12	+23	21	+6	16	+3	58	31
	10	7	50	57	12	+23	22	−4	57	+4	52	19
	10	9	55	52	42	+23	22	−1	7	+4	51	39
	16	7	7	63	9	+23	25	−7	47	+7	55	29
				62	48						55	8
	16	9	55	65	0	+23	25	−7	23	+7	57	45

As we have no particular reaſon to ſelect one meaſure rather than another, a mean of all the 13 will probably be neareſt the truth; ſo that by theſe obſervations, which, as we ſaid before, are reduced to the 4th of October, 1783, we find the poſition of the axis of Mars that day to have been 55° 41′ ſouth following.

From the appearances of the ſouth polar ſpot in 1781, repreſented fig. 27. we may conclude, that its center was nearly polar. We find it continued viſible all the time Mars revolved on its axis; and, to preſent us generally with a pretty equal ſhare of the luminous appearance, a ſpot which covered from 45° to 60° of a great circle on the globe of Mars could not have any conſiderable polar diſtance: however, a ſmall correction in the angle of poſition ſeems to be neceſſary, which ſhould be taken from the meaſure of the 15th of July, becauſe that branch of the ſpot which probably extended fartheſt towards the

the equator, was then in the *following* quadrant. The meaſure of both the ſpots on June the 25th, 1781, is ſtill more to be depended on, as giving us very nearly the poſition of the true pole; for it appears evident from the phænomena of the bright north-polar ſpot in fig. 26. that that ſpot was in the meridian when the meaſure was taken, while the ſouthern ſpot was in the *preceding* quadrant near its greateſt limit. Now, ſince an angle at the circumference of a circle is but half the angle at the center, when the arches which ſubtend theſe angles are equal, the correction neceſſary to be applied to the meaſure taken through the two ſpots will be but one half of the correction which would have been requiſite had it been taken through the center; therefore, in order to reduce this to the condition of the former, we may ſuppoſe it to have been taken through the center of Mars when the ſpot was only 30, or 150 degrees from the meridian. It is alſo neceſſary to add 1° 54′ to the angle of July 15, which it would have meaſured more had the planets remained where they were June 25. This done, we may have the polar diſtance of the center of the ſpot as before. Half the ſum of the ſines (of 231° 38′ and 150°) to radius, as 50′ (half the difference between 74° 32′ and 76° 12′) to a fourth number, which is 1° 18′.

I ſhould obſerve here, that the meaſures of the angle of poſition would be too large before the ſpot came to the meridian, and too ſmall afterwards, the axis of Mars being ſouth preceding; whereas, in fig. 28, they would be too ſmall before, and too large after, the meridian paſſage, the pole being ſouth following.

Theſe two obſervations arranged as thoſe in the fourth table, and reduced to the time of the 25th of June, will ſtand as follows.

TABLE

TABLE V.

Time of obſervation.	Angles taken.	Angle Q.	First correction.	Second correct.	Corrected Angle.
D. H. M.	D. M.	D. M.		D. M	D. M.
June 25 11 36	74 32	−10 14	+ { half of 1° 18′	−0 0	75 11
July 15 10 12	74 18	− 8 20	−1 1	+1 54	75 11

I am to remark, that we have here admitted both meaſures as equally good; and that, therefore, the reſult is a mean of them both, and ſhews the axis of Mars, June 25, 1781, to have been 75° 11′ ſouth preceding.

Our next buſineſs will be to reduce theſe two geocentric obſervations to a heliocentric meaſure. This is to be done, as we have ſhewn before, by a calculation of the angle Q, fig. 29. The reſult of it ſhews, that 10° 14′ are to be ſubtracted from the mean corrected angle of poſition, reduced to June 25, 1781, and 23° 18′ to be added to the angle which is the corrected mean of 13 meaſures, reduced to Oct. 4, 1783. Hence we learn, that on thoſe days and hours, when the heliocentric places of Mars were 9 s. 24° 35′, and 0 s. 7° 15′ (which would happen about July 18, 1781, and Sept. 29, 1783) an obſerver placed in the ſun would have ſeen, on the former, the axis of Mars inclined to the ecliptic 64° 57′, the north pole being towards the left; and on the latter, he would have ſeen the ſame axis inclined to the ecliptic 78° 59′, the north pole being then towards the right.

The firſt concluſion we may draw from theſe principles is, that the north pole of Mars muſt be directed towards ſome point of the heavens between 9 s. 24° 35′ and 0 s. 7° 15′; becauſe the change of the ſituation of the pole from left to right, which

which happened in the time the planet paſſed from one place to the other, is a plain indication of its having gone through the node of the axis. Next, we may alſo conclude, that the node muſt be conſiderably nearer the latter point of the ecliptic than the former; for, whatever be the inclination of the axis, it will be ſeen under equal angles at equal diſtances from the node.

But, by a trigonometrical proceſs of ſolving a few triangles, we ſoon diſcover both the inclination of the axis, and the place where it interſects the ecliptic at rectangles (which, for want of a better term, I have perhaps improperly called its node). Accordingly I find, by calculation, that the node is in 17° 47′ of Piſces, the north pole of Mars being directed towards that part of the heavens; and that the inclination of the axis to the ecliptic is 59° 42′.

We ſhall now compare the obſervations of an earlier date with theſe principles, to ſee how far they agree. Some of the particulars and calculations relating to them are as follow,

TABLE VI.

Times of Obſervation.	Eſtimations.	Geoc. longit.	Angle Q_4	ad correct.
D. H. M.	D.	S. D. M.	D. M.	
1779, May 9 12 0	42	7 22 20	+ 14 45	+ 0
May 11 12 0	62	7 21 40	+ 15 11	+ 26
1777, Apr. 17 7 50	63	6 3 34	+ 23 26	

May the 9th, 1779, as we have ſeen, the angle of poſition was roughly eſtimated at 42°, and May 11. at 62°. The great diſagreement of theſe coarſe eſtimations is undoubtedly owing to the very different ſituation of the dark ſpot from which they

were taken; however, ſince we do not mean to uſe theſe obſervations in our calculations, they may ſuffice in a general way to ſhew, that the axis of Mars was actually about that time in ſuch a ſituation as our principles give it: for, reducing the two poſitions to the 9th of May, that of the 11th, from an allowance of 26′ for the ſituation of the planets, will become 62° 26′; and a mean of the two, 50° 13′ ſouth preceding; which, reduced to a heliocentric obſervation, gives 66° 30′, the north pole lying towards the left. Now, on calculating from the poſition of the node and inclination of the axis before determined, we find, that the heliocentric angle was 62° 49′, the north pole pointing towards the left; and a nearer agreement with theſe principles could hardly be expected from eſtimations ſo coarſe. If we go to the year 1777, and take the poſition of the two bright ſpots obſerved the 17th of April, we have 63° ſouth preceding; this, reduced to a heliocentric quantity, gives 86° 26′ of inclination, the north pole being to the left. By calculating we find, that that pole was then actually 81° 27′ inclined to the ecliptic, and pointed towards the left as ſeen from the ſun.

The inclination and ſituation of the node of the axis of Mars with reſpect to the ecliptic being found may thus be reduced to that planet's own orbit. Let EC, fig. 30. (tab. X.) be a part of the ecliptic; OM part of the orbit of Mars; PEO a line drawn from P, the celeſtial pole of Mars, through E, that point which has been determined to be the place of the node of the axis of Mars in the ecliptic, and continued to O where it interſects the orbit of Mars. Now, if according to Mr. DE LA LANDE we put the node of the orbit of Mars for 1783, in 1 s. 17° 58′, we have from the place of the node of the axis (that is, 11 s. 17° 47′) to the place of the node of the orbit,

an arch EN of 60° 11′; in the triangle NEO, right-angled at E, there is also given the angle ENO, according to the same author, 1° 51′, which is the inclination of the orbit of Mars to the ecliptic. Hence we find the angle EON 89° 5′, and side ON 60° 12′. Again, when Mars is in the node of its orbit N, we have, by calculation from our principles, the angle PNE = 63° 7′, to which, adding the angle ENO = 1° 51′, we have PNO = 64° 58′; from which two angles PON and PNO with the distance ON, we obtain the inclination of the axis of Mars, and place of its node with respect to that planet's own orbit; the inclination being 61° 18′, and the place of the node of the axis 58° 31′ preceding the intersection of the ecliptic with the orbit of Mars, or in our 19° 28′ of Pisces.

Being thus acquainted with what the inhabitants of Mars will call the obliquity of their ecliptic, and the situation of their equinoctial and solstitial points, we are furnished with the means of calculating the seasons on Mars; and may account, in a manner which I think highly probable, for the remarkable appearances about its polar regions.

But first it may not be improper to give an instance how to resolve any query concerning the martial seasons. Thus, let it be required to compute the declination of the Sun on Mars, June 25, 1781, at midnight of our time. If ♈ ♉ ♊ ♋, &c. fig. 31. (tab. X.) represent the ecliptic of Mars, and ♈ ♍ ♎ ♓ the ecliptic of our planet, A*a*, *b*B, the mutual intersection of the martial and terrestrial ecliptics, then there is given the heliocentric longitude of Mars, ♈*m* = 9 s. 10° 30′; then taking away six signs, and ♎ *b*, or ♈*a* = 1 s. 17° 58′, there remains *bm* = 1 s. 22° 32′. From this arch, with the given inclination, 1° 51′, of the orbits to each other, we have cosine of inclination to radius, as tangent of *bm* to tangent of BM = 1 s. 22° 33′. And

taking away B ♈ = 1 s. 1° 29′, which is the complement to ♈ B (or ☊ A, already ſhewn to be 1 s. 28° 31′) there will remain ♈ M = 0 s. 21° 4′, the place of Mars in its own orbit *; that is, on the time abovementioned, the ſun's longitude on Mars will be 6 s. 21° 4′, and the obliquity of the martial ecliptic 28° 42′ being alſo given, we find, by the uſual method, the ſun's declination 9° 56′ ſouth.

The analogy between Mars and the earth is, perhaps, by far the greateſt in the whole ſolar ſyſtem. Their diurnal motion is nearly the ſame; the obliquity of their reſpective ecliptics, on which the ſeaſons depend, not very different; of all the ſuperior planets the diſtance of Mars from the ſun is by far the neareſt alike to that of the earth: nor will the length of the martial year appear very different from that which we enjoy, when compared to the ſurpriſing duration of the years of Jupiter, Saturn, and the Georgium Sidus. If, then, we find that the globe we inhabit has its polar regions frozen and covered with mountains of ice and ſnow, that only partly melt when alternately expoſed to the ſun, I may well be permitted to ſurmiſe that the ſame cauſes may probably have the ſame effect on the globe of Mars; that the bright polar ſpots are owing to the vivid reflection of light from frozen regions; and that the reduction of thoſe ſpots is to be aſcribed to their being expoſed to the ſun. In the year 1781, the ſouth polar ſpot was extremely large, which we might well expect, ſince that pole had but lately been involved in a whole twelvemonth's darkneſs and abſence of the ſun; but in 1783 I found it conſiderably ſmaller than before, and it decreaſed continually

* If no very great accuracy be required, we may add 3 s. 10° 34′ to any given place of our ecliptic, which will at once reduce it to what it ſhould be called on the orbit of Mars, and will always be true to within a minute.

from

from the 20th of May till about the middle of September, when it ſeemed to be at a ſtand. During this laſt period the ſouth pole had already been above eight months enjoying the benefit of ſummer, and ſtill continued to receive the ſun-beams; though, towards the latter end, in ſuch an oblique direction as to be but little benefited by them. On the other hand, in the year 1781, the north polar ſpot, which had then been its twelve-month in the ſun-ſhine, and was but lately returning to darkneſs, appeared ſmall, though undoubtedly increaſing in ſize. Its not being viſible in the year 1783 is no objection to theſe phænomena, being owing to the poſition of the axis, by which it was removed out of ſight; moſt probably, in the next oppoſition we ſhall ſee it renewed, and of conſiderable extent and brightneſs; as, by the poſition of the axis of Mars, the ſun's ſouthern declination will then be no more than 6° 25′ on that planet.

Of the ſpheroidical figure of Mars.

That a planetary globe, ſuch as Mars, turning on an axis, ſhould be of a ſpheroidical form, will eaſily find admittance, when two familiar inſtances in Jupiter and the earth, as well as the known laws of gravitation and centrifugal force of rotatory bodies, lead the way to the reception of ſuch doctrines. So far from creating difficulties or doubts, it will rather appear ſingular, that the ſpheroidical form of this planet, which the following obſervations will eſtabliſh, has not already been noticed by former aſtronomers; and yet, reflecting on the general appearances of Mars, we ſoon find that opportunities for making obſervations on its real form cannot be very frequent: for, when it is near enough to view it to an advantage, we ſee it

generally gibbous, and its oppoſitions are ſo ſcarce, and of ſo ſhort a duration, that in more than two years time we have not above three or four weeks for ſuch obſervations. Beſides, aſtronomers being already uſed to ſee this planet generally diſtorted, the ſpheroidical form might eaſily be overlooked.

Obſervations relating to the polar flattening of Mars.

1783, Sept 25. 9 h. 50′. I can plainly ſee that the equatorial diameter of Mars is longer than the polar. Meaſure of the equatorial diameter 21″ 53‴; of the polar diameter 21″ 15‴ *full meaſure*, that is, certainly not too ſmall. The wires were ſet as outward tangents to the diſk, and the zero, as well as the meaſures, were taken by the light of Mars.

Sept. 28. 14 h. 25′. I ſhewed the difference of the polar and equatorial diameters of Mars to Mr. WILSON, Aſſiſtant Profeſſor of Aſtronomy at Glaſgow. He ſaw it perfectly well, ſo as to be entirely convinced it was not owing to any defect or diſtortion occaſioned by the eye lens; and, becauſe I wiſhed him to be ſatisfied of the reality of the appearance, while he was obſerving, I reminded him of ſeveral well known precautions; ſuch as cauſing the planet to paſs directly through the center of the field of view, and judging of its figure at the time when it was moſt diſtinct and beſt defined, and ſo forth.

Sept. 29. I ſhewed the difference of the polar and equatorial diameters of Mars to Dr. BLAGDEN and Mr. AUBERT. Dr. BLAGDEN not only ſaw it immediately,

diately, but thought the flattening almoſt as much as that of Jupiter. Mr. AUBERT alſo ſaw it very plainly, ſo as to entertain no manner of doubt about the appearance.

As we cannot take too many opportunities of confirming our own obſervations by the eyes of other obſervers, I eſteemed it a very fortunate circumſtance to have the honour of a viſit from theſe gentlemen at ſo particular a time, Mars being this day within 37 hours of the oppoſition, and yeſterday when Mr. WILSON ſaw it, within about two days and a half.

1783, Sept. 30. 10 h. 52′. The difference in the diameters of Mars is very evident and conſiderable.

Meaſure of the equatorial diameter 22″ 9‴ with 278.
Second meaſure - - 22″ 3‴ full large.
Polar diameter very exact - 21″ 26‴.

Oct. 1. 10 h. 50′. I took meaſures of the diameters of Mars with my 20-feet reflector. The equatorial meaſured 103 parts of the micrometer; the polar 98. The value of the diviſions in ſeconds and thirds not being well determined, on account of ſome late change in the focal length of the ſeveral 20-feet object metals I uſe, we have only from theſe meaſures the proportion of the diameters as 103 to 98.

13 h. 15′. Every circumſtance being favourable, I took the following meaſures of the diameters of Mars with my 7-feet reflector, and a diſtinct power of 625.

Equatorial diameter 22″ 12‴ narrow meaſure.
22″ 46‴ rather full.
22″ 35‴ exact.

Polar

Polar diameter 21″ 24‴
21″ 33‴ very exact.

I ſaw Mars perfectly well all the time I meaſured, with all its figures upon the diſk appearing diſtinctly; and, I think, theſe meaſures may be depended upon better than any I have yet taken.

1783, Oct. 5. 14 h. 0′. The difference of the diameters is very ſenſible.

Oct. 7. 9 h. 43′. The flattening of the poles is very viſible.

13 h. 40′. I turned my Newtonian 7-feet reflector one quarter round, ſo as to bring the place to look in at to the bottom; and, as well as the uneaſy poſture would allow, I ſaw the flattening of the poles the ſame as when I looked in at the ſide; power 460.

14 h. 30′. With a 3½ feet achromatic teleſcope and a ſingle eye lens, I ſaw the difference of the polar and equatorial diameters very plainly.

Oct. 9. 8 h. 40′. I turned my reflector 90° round, ſo as now to look in at the upper end, but ſaw not the leaſt difference in appearances; for, returning it again immediately to its uſual poſition, in both caſes the equatorial diameter appeared a little longer than the other; power 278, and the evening fine.

I turned the great ſpeculum one quadrant in its cell, but appearances were not in the leaſt altered; the equatorial diameter ſtill was a little longer than the polar one.

I tried a very fine new object ſpeculum, and found alſo the equatorial diameter a little longer than the polar one.

1783,

1783, Oct. 9. 10 h. 47′. The flattening at the poles very visible.

Oct. 10. 9 h. 55′. A little of the polar flattening is visible, so as to admit of no doubt; power 460, very distinct.

11 h. 32′. Mars visibly flattened, but not much; the achromatic shews it also.

11 h. 42′. The disk of Mars is visibly spheroidical.

Oct. 11. 7 h. 37′. Mars is plainly gibbous, therefore measures and estimations of the diameters must for the future be improper.

11 h. 12′. It is rather difficult to say of what shape Mars is now, for it is partly flattened and partly gibbous; but the gibbous side not being quite in the polar direction of Mars, this produces altogether an odd mixture of shapes: however, upon the whole, the polar diameter is still rather the smallest.

11 h. 13′. The *preceding* side of Mars shews the flattening of the poles, while the *following* is terminated by an elliptical arch.

Oct. 12. 11 h. 12′. The flattening upon the whole is visible.

Oct. 17. 13 h. 7′. The effect of gibbosity is scarcely equal to the flattening; or, upon the whole, the planet is still rather broader over the equator than over the poles.

Nov. 1. 7 h. 56′. The semi-disk, which is *full*, is evidently part of an oblate spheroid; but, to an eye not attentively looking for it, and knowing the shape and exact situation of the poles of Mars, this would probably not appear.

1783, Nov. 10. 9 h. 30'. The gibboſity of Mars is now ſuch, that the polar diameter is conſiderably longer than the equatorial; but the deficiency not being exactly from pole to pole, makes the diſk of a crooked, irregular figure, and renders preciſion in this eſtimation impoſſible; otherwiſe the phaſe of Mars would have made a pretty good micrometer upon the equatorial diameter, and it was with ſuch a view I had directed my attention to this circumſtance: appearances, however, are viſibly in favour of the polar diameter's being the longeſt.

We find that the quick alterations in the viſible diſk of Mars, during the time it is in the beſt ſituation for us to obſerve it, are ſuch, that if we were to uſe many meaſures which have been taken of its diameters, we ſhould be obliged to have recourſe to a computation of its phaſes, in order to make proper allowance for them. Now, ſince theſe changes are in a longitudinal direction, and the poles of Mars are not perpendicular to the ecliptic, it would bring on a calculation of ſmall quantities, which it is always beſt not to run into where it can be avoided. For this reaſon, I ſhall at once ſettle the proportion of the equatorial to the polar diameter of this planet, from the meaſures which were taken on the very day of the oppoſition. I prefer them alſo on another account, which is, that they were made in a very fine, clear air, and were repeated with a very high power, and with two different inſtruments, of whoſe faithful repreſentation of celeſtial objects, the many obſervations on very cloſe double ſtars I have made with them have given me very evident proofs.

As

As we are at preſent only in queſt of the proportion of one diameter to the other, the meaſures of the 20-feet reflector, though not given in angular quantities, will equally ſuffice for the purpoſe. By them we have the equatorial diameter to the polar as 103 to 98, or as 1355 to 1289. I have turned the proportion into the latter numbers by way of comparing them the better with the meaſures of the 7-feet reflector. By that inſtrument the equator of Mars, Oct. 1. we find, was meaſured three times; but from the remarks annexed to the different reſults, I think the third meaſure ſhould be uſed. Indeed, on taking the difference of the two firſt. which is 34‴, and dividing by three, we have the quotient $11\frac{1}{3}$‴; then, allotting two-thirds to the firſt, becauſe the remark ſays poſitively "narrow meaſure," it becomes 22″ $34\frac{2}{3}$‴, and taking one-third from the ſecond, which is expreſſed doubtfully, "rather too full," it becomes 22″ $35\frac{1}{3}$‴: this reflection on the two firſt meaſures gives additional validity to the third, which is 22″ 35‴, or 1355‴. The polar diameter was meaſured twice; and as no reaſon appears againſt either of the obſervations, I ſhall take the mean of both, which is 21″ 29‴, or 1289‴; ſo that by theſe meaſures the equatorial diameter of Mars is to the polar as 1355 to 1289. A leſs perfect agreement between the proportions of the diameters ariſing from the meaſures of the 20-feet reflector and thoſe which we have juſt now deduced from the 7-feet, would have been ſufficient for our purpoſe, as we might eaſily have excuſed one or two thouſandths of the whole quantity; however, we have no cauſe to be diſpleaſed with this coincidence, though it ſhould in part be owing to accident, and therefore ſhall admit the above proportion, and proceed to a farther examination of it.

In the firſt place, it will be neceſſary to ſee whether any correction be required on account of the different heliocentric and geocentric ſouth latitude of Mars; which would apparently compreſs the polar diameter a little, by the defect of illumination on the north. On computation we find, that a difference ariſing from that cauſe would give the longitudinal diameter to the latitudinal as 20000 to 19987; which being much leſs than one thouſandth part of the whole, may therefore be neglected.

But next, a very conſiderable correction muſt be admitted, when we take into account the poſition of the axis of Mars. The declination of the ſun on that planet, at the time the meaſures were taken, was not leſs than 27° ſouth; ſo that the poles were not in the circumference of the diſk by all that quantity. On a ſuppoſition then, that the figure of Mars is an elliptical ſpheroid, we are now to find the real quantity of the polar diameter from the apparent one. It has been proved, that, in the ellipſis, the exceſſes of any diameters above the polar one are as the ſquares of the coſines of the latitudes *; but the diameter at rectangles to the equator of Mars, which was expoſed to our view in the late oppoſition, was not the polar one, but ſuch as muſt take place in a latitude of 63°. Putting therefore m = coſine of 63°, $a = 1355$, $b = 1289$, $x =$ the polar axis, we have $1 : m^2 :: a - x : b - x$. And $\frac{b - m^2 a}{1 - m^2} = x$; which gives us 1272 nearly, for the polar diameter. The true proportion, therefore, of the equatorial to the polar diameter will be as 1355 to 1272; which, reduced to ſmaller but leſs accurate numbers, is 16 to 15 nearly.

* Aſtr. par M. DE LA LANDE, § 2680.

I ſhall

I ſhall now alſo mention ſome of the other meaſures, but with a view only to ſhew that they are very conſiſtent with the above determination. From thoſe of the 30th of September, for inſtance, we collect the proportion of the diameters of Mars as 1340 to 1286; or, reduced to our former numbers, 1355 to 1300. Now, ſince theſe meaſures were taken the night before the oppoſition, they muſt on that account be as good as the former; and, had thoſe of the day of oppoſition not been preferred, becauſe they were oftener repeated, and the ſuperior power of the 7, and great light of the 20-feet reflector, gave them additional weight, I ſhould have taken them into the account; the very ſmall difference, however, cannot but ſtrengthen the reſults of the former meaſures.

From the obſervations of the 25th of September we have the proportion of the diameters as 1313 to 1275; and if the equatorial meaſure be increaſed in the ratio of 20000 to 19953, on account of the different heliocentric and geocentric longitude, Mars not being at the full, it will give the ratio of 1316 to 1275; or, conforming to our former numbers, as 1355 to 1312. I have not been very ſtrict in the application of the correction deduced from the phaſes of Mars, ſince no other uſe was intended to be made of theſe numbers than merely to ſhew, that they do not very greatly differ from thoſe we have aſſigned before *.

It

* If more ſtrictneſs be required, let EC, fig. 32. be the ecliptic; PS its poles; *ps* the poles of Mars, and *eq* its equator. Then, the angle *pm*C being found, by calculation, we ſhall have C*m* (radius) to *cm* (coſine of the difference between the heliocentric and geocentric longitude) as *qv* (ſine of the angle *qmv* or *pm*C) to *ov*. Then, ſince with Mars C*c* can never be very great, the ſmall triangle *qno* may be taken for ſimilar to *qvm*; therefore *qm* (radius) is to *qv* (ſine of *pm*C).

It was obſerved, Oct. 17, 1783, that the equatorial diameter of Mars was ſtill greater than the polar, notwithſtanding the depredation of the defect of light upon it. On calculating the phaſes, we find, that the longitudinal diameter was, that day, to the latitudinal one as 19711 to 20000, which therefore could not be an equal balance to oppoſe the ſpheroidical figure ſo as to render it inviſible.

But, Nov. 10. the proportion of the longitudinal diameter to the latitudinal one, from a computation of the phaſe of Mars, muſt have been as 18762 to 20000; and accordingly it was by obſervation found to be more than ſufficient to take off all appearance of the polar flattening, and leave a viſible exceſs in the axis above the equator.

To obviate any doubts concerning a fallacy that might ariſe from the convexity of the eye-glaſs, or irregular ſhape of the ſmall ſpeculum, I need only refer, for the latter, to the experiments of the 7th and 9th of October, 1783: for ſhould the ſhort diameter of my ſmall plane ſpeculum have occaſioned a compreſſing of the polar diameter of Mars when expoſed to it, half a turn of the teleſcope muſt bring the other diameter of that ſpeculum into the ſame ſituation, and a contrary effect would have followed. With regard to the former, not only the experiments made with the achromatic, but principally the obſervation with the 20-feet reflector, where I uſed a compound eye-piece magnifying only about 300 times, will ſufficiently exculpate the eye-glaſſes. It is alſo well known, that in a ſingle lens the diſtortion of the images, if any ſuch there

pmC) as qo ($=qv-vo$) to qn; which is the required correction or deficiency of the equatorial diameter eq of Mars.

Or, putting $mC=1$ and $vq=m=$ coſine of the angle Pmp; it will be $qn=m^2 \cdot eC$.

ſhould be, will equally affect the wires of the micrometer, and give a true meaſure notwithſtanding; and the compound eye-piece I uſed with the 20-feet reflector had likewiſe the ſame advantage, for it is conſtructed on the plan lately propoſed by Mr. RAMSDEN in the Philoſophical Tranſactions*, which he was ſo obliging as to communicate to me about a twelve-month ago, and which I immediately adapted to my large micrometers.

On the ſubject of the figure of Mars I ought to remark alſo, that perhaps the meaſures which were taken of its diameters during the laſt oppoſition will enable us to aſcertain its real ſize with greater accuracy than has been done before. The micrometer which can diſtinguiſh with preciſion between the equatorial and polar diameters of this ſmall planet, will certainly be admitted as an evidence of conſiderable conſequence; and ſince the reſult of theſe meaſures is pretty different from what former obſervations give us, I ſhould not omit mentioning it.

We have ſeen that the equatorial diameter, on the day of the oppoſition, meaſured 22″ 35‴. The diſtance of Mars from the earth at that time was .40457, the mean diſtance of the earth from the ſun being 1; therefore, 22″ 35‴ reduced to the ſame diſtance will be no more than 9″ 8‴.

I ſhall conclude this ſubject with a conſideration relating to the atmoſphere of Mars. Dr. SMITH † reports an obſervation of CASSINI's, where "a ſtar in the water of Aquarius, at the "diſtance of ſix minutes from the diſk of Mars, became ſo "faint before its occultation, that it could not be ſeen by the "naked eye, nor with a 3-feet teleſcope." It is not men-

* Vol. LXXIII. p. 94.

† Optics, § 1096.

tioned

tioned what was the magnitude of the ſtar; but, from the circumſtance of its becoming inviſible to the naked eye, we may conclude, that it muſt have been of the ſixth or ſeventh magnitude at leaſt. The reſult of this obſervation would indicate an atmoſphere of ſuch an extraordinary extent, ſince at the diſtance of 36 ſemi-diameters of the planet it ſhould ſtill be denſe enough to render ſo conſiderable a ſtar inviſible, that it will certainly not be amiſs to give an obſervation or two which ſeem of a very different import.

1783, Oct 26. There are two ſmall fixed ſtars preceding Mars, of different ſizes; with 460 they appear both duſky red, and are pretty unequal; with 278 they appear conſiderably unequal. The diſtance from Mars of the neareſt, which is alſo the largeſt, with 227 meaſured 3′ 26″ 20‴. Some time after, the ſame evening, the diſtance was 3′ 8″ 55‴, Mars being retrograde. I ſaw them both very diſtinctly. I viewed the two ſtars with a new 20-feet reflector of 18,7 inches aperture, and found them, as I expected, very bright.

Oct. 27. I ſee the two ſmall ſtars again. The ſmall one is not quite ſo bright in proportion to the large one as it was laſt night, being a good deal nearer to Mars, which is now on the ſide of the ſmall ſtar; but when I draw the planet aſide, or out of view, I ſee it then as well as I did laſt night. The diſtance of the ſmall ſtar meaſured 2′ 56″ 25‴ *.

* The meaſures were accurate enough for the purpoſe, though not otherwiſe to be depended on nearer than, perhaps, ſix or eight ſeconds.

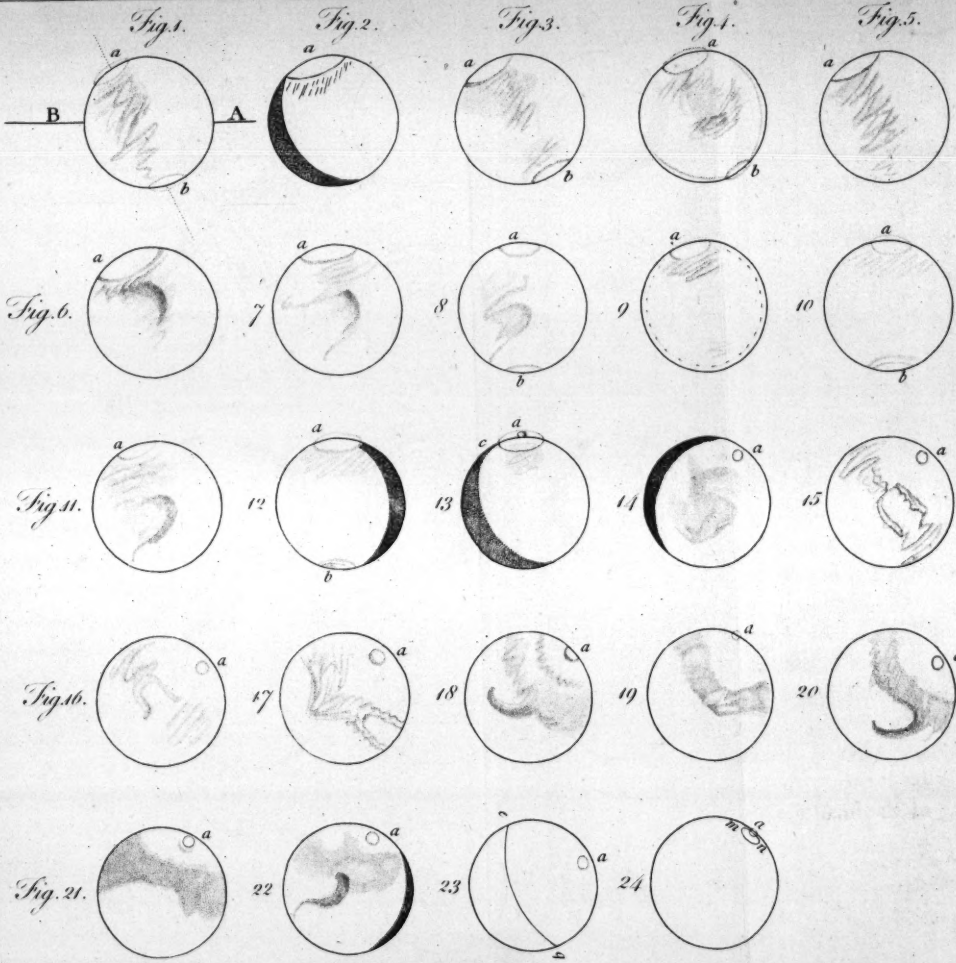
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
B
A
a
b
Fig. 6.
7
8
9
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Fig. 11.
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Fig. 16.
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Fig. 21.
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23
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m
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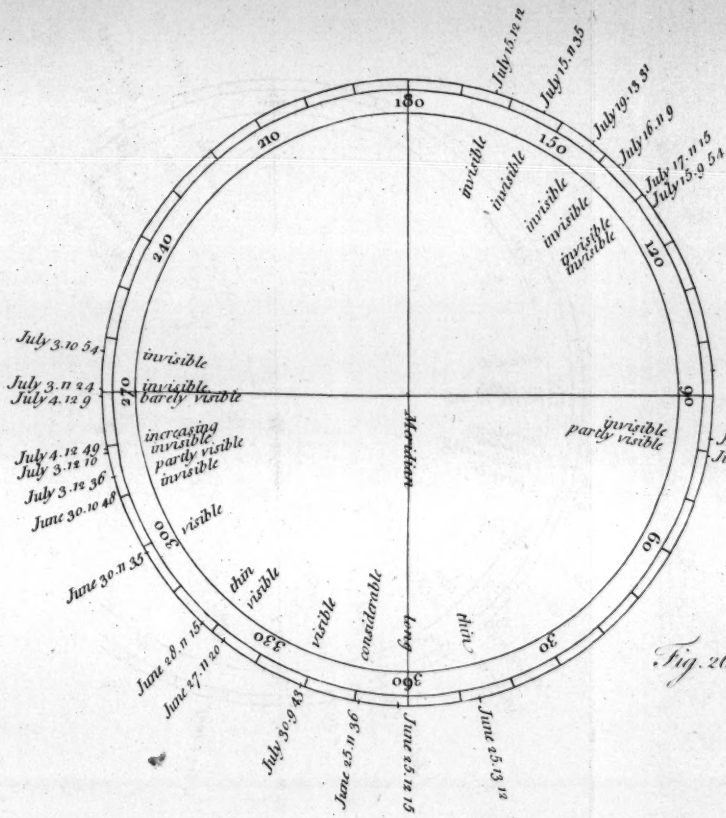


Fig. 26.

Track of the bright north polar spot on Mars, in June & July 1781.

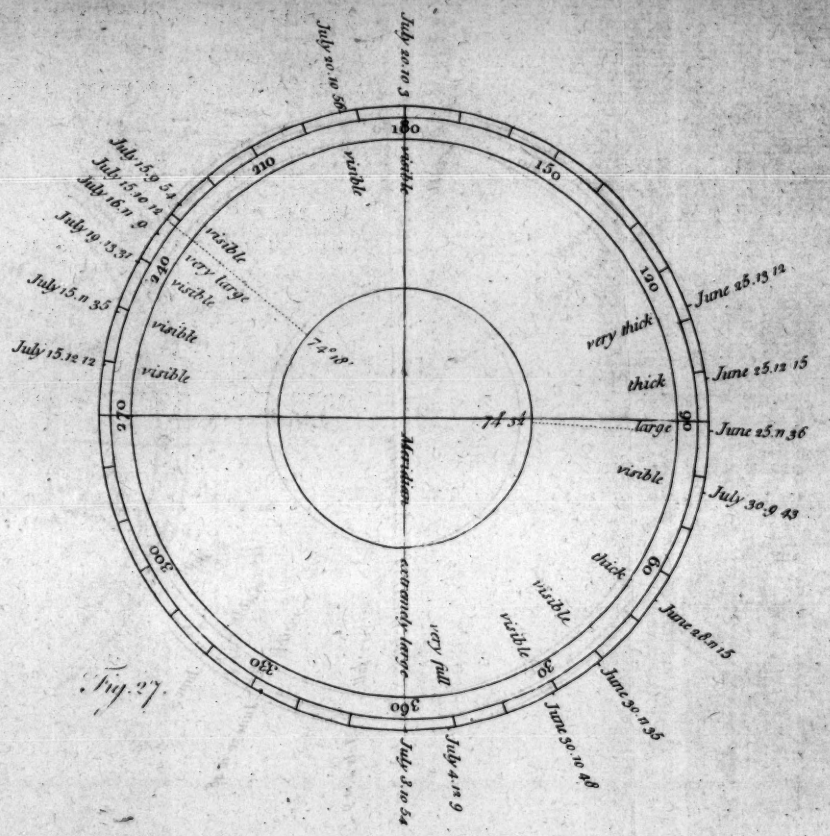


Fig. 27.

Track of the bright south polar spot on Mars, in June & July 1781.

Basire Sc.

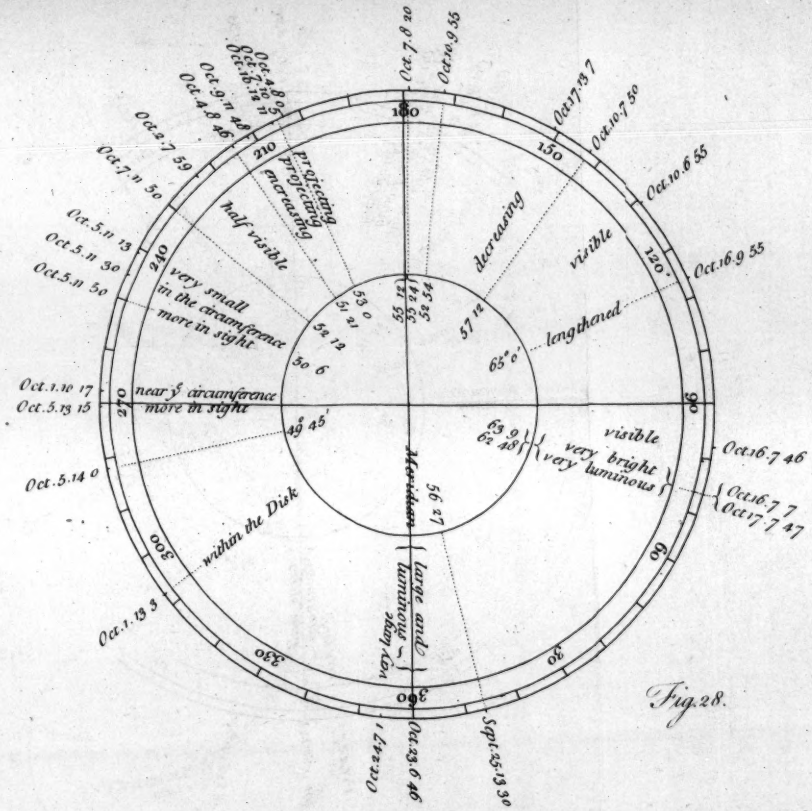


Fig. 28.

Track of the bright South polar spot on Mars, in October 1783.

Bayer fc

Fig. 25.

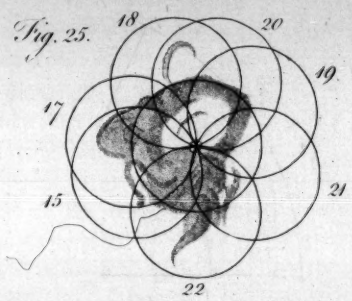


Fig. 29.

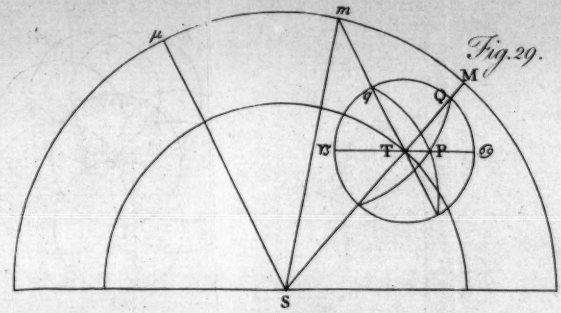


Fig. 30.

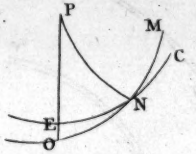


Fig. 31.

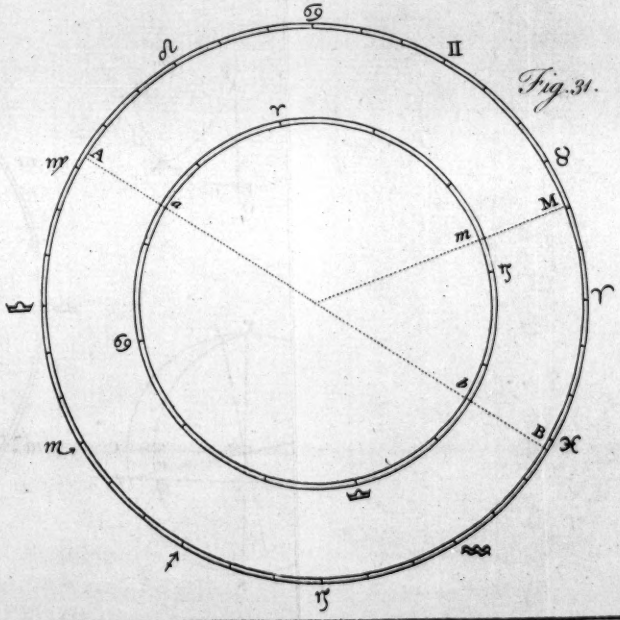
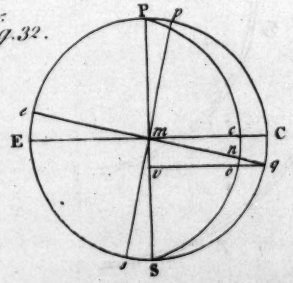
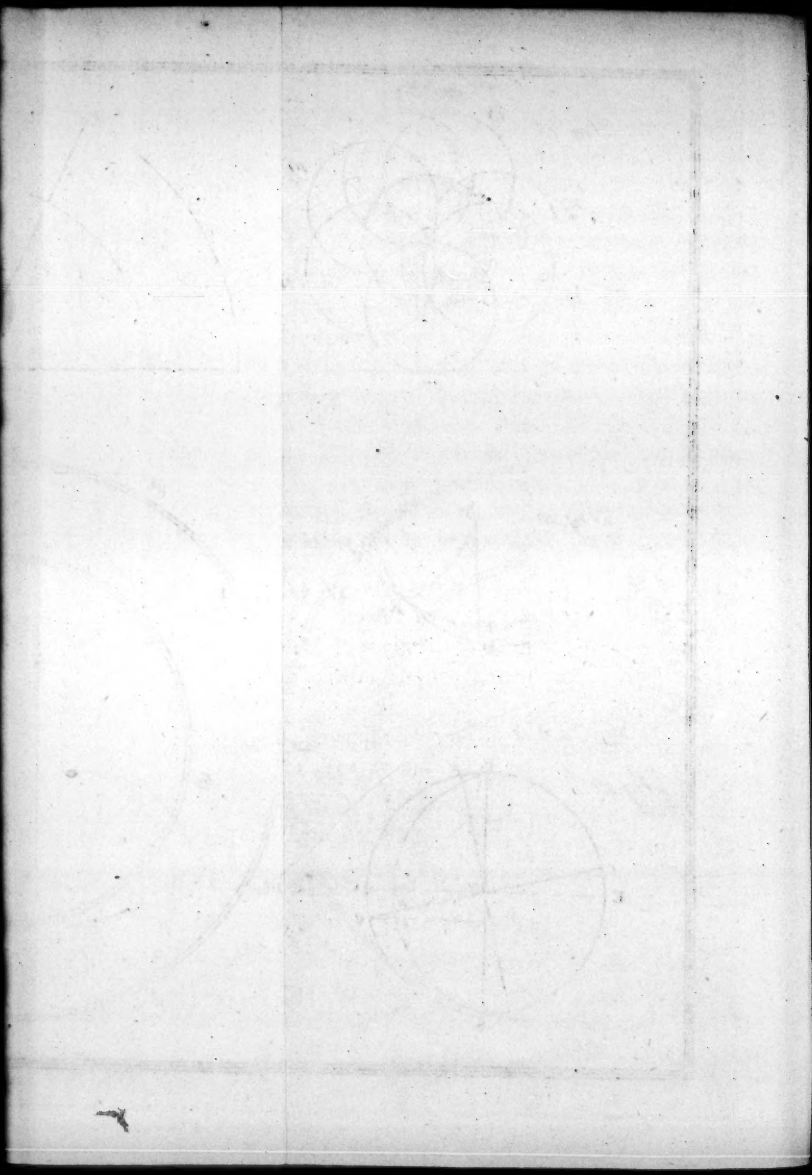


Fig. 32.



Sayers sc.



The largeſt of the two ſtars on which the above obſervations were made cannot exceed the twelfth, and the ſmalleſt the thirteenth or fourteenth magnitude; and I have no reaſon to ſuppoſe that they were any otherwiſe affected by the approach of Mars, than what the brightneſs of its ſuperior light may account for. From other phænomena it appears, however, that this planet is not without a conſiderable atmoſphere; for, beſides the permanent ſpots on its ſurface, I have often noticed occaſional changes of partial bright belts, as in fig. 1. and 14.; and alſo once a darkiſh one, in a pretty high latitude, as in fig. 18. And theſe alterations we can hardly aſcribe to any other cauſe than the variable diſpoſition of clouds and vapours floating in the atmoſphere of that planet.

Reſult of the contents of this paper.

The axis of Mars is inclined to the ecliptic 59° 42′.

The node of the axis is in 17° 47′ of Piſces.

The obliquity of the ecliptic on the globe of Mars is 28° 42′.

The point Aries on the martial ecliptic anſwers to our 19° 28′ of Sagittarius.

The figure of Mars is that of an oblate ſpheroid, whoſe equatorial diameter is to the polar one as 1355 to 1272, or as 16 to 15 nearly.

The equatorial diameter of Mars, reduced to the mean diſtance of the earth from the ſun, is 9″ 8‴.

And that planet has a conſiderable but moderate atmoſphere, ſo that its inhabitants probably enjoy a ſituation in many reſpects ſimilar to ours.

Datchet, Dec. 1, 1783. W. HERSCHEL.

XX. *A Deſcription of the Teeth of the* Anarrhichas Lupus Linnæi, *and of thoſe of the* Chætodon nigricans *of the ſame Author; to which is added, an Attempt to prove that the Teeth of cartilaginous Fiſhes are perpetually renewed. By Mr.* William Andre, *Surgeon; communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read March 18, 1784.

THE amazing variety there is in the external form of fiſhes muſt be obvious to a common obſerver; and whoever examines will be convinced, that the ſame variety prevails in their internal ſtructure. No parts, perhaps, afford a more convincing proof of the laſt aſſertion than the teeth of fiſhes. To adduce a few inſtances, let us only recollect the tuberculated teeth in the thorn-back; the triangular ſerrated teeth in the ſhark; the ſlender flexible teeth in the *chætodontes*, or angel-fiſhes. There is not only a difference of their form, but alſo in the ſubſtances of which they are compoſed; ſome being of a ſoft horny nature; others made up of bone; others of that ſubſtance we call enamel in the teeth of quadrupeds; and ſome having the apparent hardneſs and tranſparency of cryſtal. We may alſo notice their uncommon ſituation; many fiſhes having teeth not only in their jaws, but on the tongue, the palate, and about the *fauces*.

To illuſtrate in ſome degree this part of natural hiſtory, I ſhall deſcribe the teeth of the *Anarrhichas Lupus*, or Sea-wolf, and thoſe of the *Chætodon nigricans*, a ſpecies of Angel-

Angel-fiſh. The former have been but imperfectly deſcribed, and never repreſented diſtinct from the fiſh, without which it is impoſſible to have any exact idea of their diſpoſition, number, or form, while the true ſhape and compoſition of the latter, from their minuteneſs, have been entirely overlooked. I ſhall then attempt to prove, that a continual renovation of the teeth takes place in cartilaginous fiſhes.

THE SEA-WOLF is a fierce and ravenous fiſh, as its name imports, found in the northern parts of the globe, where it frequently grows to the length of four feet and upwards.

The jaws of the Wolf-fiſh are made up of ſeveral bones, to each of which a greater or leſs number of teeth are affixed; but, before I enter upon the deſcription of them, I ſhall take notice of the palate (marked A. tab. XI.), that being a kind of baſis or ſupport to the other bones, to which they are all more or leſs connected. The palate is a thick and firm bone united above to the bones of the *cranium* and noſe, and ending below in a flat oval ſurface, on which are incruſted about twelve or thirteen ſtrong, blunt, and rather flat teeth of the *molar* or grinder kind. The external edges of the teeth are the moſt prominent; by which means a hollow is formed in the middle of the palate.

The upper jaw is compoſed of three bones, two of which (BB) are placed laterally, forming the ſides of the upper jaw, and the third (C) anteriorly, making the fore-part of the jaw. The third bone may be divided through its middle into two portions; but ſince it has the appearance of one bone only, the connection being very firm, I ſhall deſcribe it agreeably to that appearance, to prevent needleſs diviſions.

The ſide bones of the upper jaw have nearly the ſhape of an italic *ſ*. At their poſterior ends may be obſerved a ſmooth

articular ſurface, for their connection with a ſimilar ſurface on the poſterior extremities of the lower jaw; and on their anterior ends there are two rows of teeth. The external row conſiſts of three or four ſharp or conical teeth; and the internal row of four or five blunt and rather flat ones. Theſe bones are connected to the palate and bones of the noſe by looſe but ſtrong ligaments.

The third bone of the upper jaw, which may be called the anterior or naſal portion, is of a triangular form, connected above to the bones of the noſe, and ending below in a flat ſurface, thick-ſet with ſharp conical teeth. The external teeth, about four in number, are large and ſtrong, and bend a little inwards; but the internal ones are ſmall, and nearly ſtraight, of which we may reckon about ten.

This bone is connected above (as I have before obſerved) to the bones of the noſe; between which a complete joint is formed, of that kind called by anatomiſts *ginglymus*, that is, where the projecting parts of one bone are received by correſponding cavities in the other. Like other articulations, it is furniſhed with a capſular ligament, and no doubt an apparatus for the ſecretion of *ſynovia*. Although a joint exiſts between this bone, and thoſe of the noſe, yet no muſcles are provided for its motion, which depends entirely upon the reſiſtance made by thoſe hard bodies which the animal takes into its mouth.

The lower jaw (D) conſiſts of two bones, united at their fore-parts by a ſtrong ligament, which allows of ſome motion. On their anterior extremities are placed ſix large and as many ſmall ſharp and conical teeth; the large teeth are placed externally, and their points are bent a little inwards; while the ſmall ones, which ſtand within them, are nearly ſtraight. Behind theſe are two or three rows of grinder teeth. The

external

external teeth ſtand nearly upright; but the internal ones are placed obliquely, inclining towards each other.

The teeth are formed of a hard bony matter, not covered with enamel as in ſome animals; nor is there an equal diſtribution of enamel and bone as in ſome others. They are not fixed in ſockets, but are faſtened to the jaws in the ſame manner as the *epiphyſes* are united to the bodies of the bones in young animals.

From the foregoing deſcription it will appear, that the anterior ſharp teeth of the Sea-wolf are admirably calculated for ſeizing its prey, while the poſterior grinding teeth ſerve to break down the hard ſhells of lobſters, crabs, muſcles, ſcollops, &c. which this animal is known to feed upon. The external teeth on the ſides of the upper and lower jaw being higher than thoſe placed within them, a hollow is formed above and below, in which the convex ſhells of cruſtaceous animals, &c. are confined during their compreſſion between the jaws, which is effected by the action of ſtrong muſcles placed on the ſides of the head. The jaws being made up of a number of pieces, and connected by looſe ligaments, a freedom of motion is allowed, and the colliſion or ſhock ariſing from the comminution of hard bodies is ſo much the leſs by being divided among a number of bones.

Merret informs us *, the *lapis huſonites* are the flat grinder teeth of this fiſh petrified. But certainly theſe foſſils are not the production of the Sea-wolf alone, ſince they may originate from all thoſe fiſhes which have flat teeth in their palate or jaws; a ſtructure which the French naturaliſts diſtinguiſh by the appellation of *palais pavé*.

* Pinax Rerum Naturalium Britannicarum.

OF THE CHÆTODON NIGRICANS.

The individual which furniſhed the following account was brought from the Weſt Indies, and meaſured about five inches in length *. Its teeth (the only parts I mean to deſcribe) were ſo ſmall as to require the aſſiſtance of a microſcope to diſcover their real ſhape. There were fourteen teeth in each jaw, ſeven of which from the upper one are repreſented tab. XII. They conſiſt of a cylindrical body fixed in the jaw, above which they ſpread out into a broad and rather flat ſurface, on the edges of which are twelve or thirteen *denticuli*, making an uncommon appearance, and totally different from the teeth of any other animal. Another ſingularity is their being tranſparent, unleſs viewed with a deep magnifier, when a few opaque lines may be perceived, which point out the cellular part of the tooth through which the blood veſſels ramify, which are deſtined for its growth and nouriſhment. They are not all of the ſame length. Thoſe in the anterior parts of the jaws are the longeſt, from whence they gradually diminiſh in length as they approach the angles of the mouth.

From the foregoing deſcription of the teeth of the *Chætodon nigricans*, this fiſh ſeems to be miſplaced in the *Syſtema Naturæ* of LINNÆUS; ſince one generic diſtinction of the *Chætodontes* is to have numerous, ſlender, and flexible teeth; whereas the teeth of the *Chætodon nigricans* are few in number, placed in one row, and of a cryſtalline hardneſs.

* This fiſh is well repreſented in DU HAMEL Traité général des Péſches, tom. III. ſeconde partie, ſection IV. planche xii. under the name of *Chirurgien ou Porte Lancette*.

Of the teeth of cartilaginous fishes.

When Steno examined the teeth of the shark, he was surprised to find a great number of them placed on the inside of each jaw, lying close to the bone, and many of them buried in a loose spongy flesh; concluding that these internal teeth could be of little or no use to the animal. Mr. Herissant * afterwards shewed the use of these internal or posterior teeth, by proving, that as the anterior teeth of each row are broken off, drop out, or wear away, the posterior ones come forward to supply their places †.

But though it be certain that the anterior teeth, when lost, are replaced by the posterior ones, neither of the above naturalists, or any other that I know of, have attempted to ascertain how often this circumstance happens. Whether the renovation be perpetual during life; or whether that operation be suspended after a limited number of teeth have been supplied.

From a singular circumstance, which I met with some time ago, I am inclined to think the former is the fact; or, that in cartilaginous fishes, such as sharks, rays, &c. there is a perpetual renovation of the teeth.

Being engaged in dissecting the jaws of a very large shark, I was surprised to find a portion of that sharp, bearded bone found in the tail of the fire-flaire, or sting-ray ‡, driven quite

* Bomare Dictionaire d'Histoire Naturelle, article Requien.

† It may not be improper on this occasion to point out a mistake which some naturalists have fallen into, in allowing a set of muscles for raising the numerous teeth placed in the jaws of sharks. I have frequently dissected the jaws of those animals, and am certain no such muscles exist, nor are they indeed at all necessary.

‡ *Raia Pastinaca* Linnæi. The French naturalists, on account of the bone in the tail, call this fish *Raie bievnnette*.

through

through the lower jaw among the poſterior teeth, and fixed almoſt immoveably. How this happened muſt be obvious to every one. (See the figure, tab. XIII.)

Before I proceed, it will be neceſſary to obſerve, firſt, that the poſterior teeth of cartilaginous fiſhes are always found in a ſoft, membranous ſtate, and but imperfectly formed; notwithſtanding this, they have the whiteneſs of teeth from a ſmall quantity of calcareous earth already depoſited within their ſubſtance. Their hardneſs and perfect form is acquired as they advance towards the anterior parts of the jaws. Secondly, that of the three angles in each tooth of the ſhark, one is placed towards the right, another towards the left, and the other, which is in the middle, and the moſt acute angle, is directed inwardly towards the tongue or *fauces*. They are placed then in ſuch a manner as that the angles of the teeth on the left-ſide in one row, approach the angles of the teeth on the right-ſide in the next row. Thoſe teeth which ſtand on a line from without inwards, I call a row; not thoſe which are placed nearly in a parallel line from one ſide of the mouth to the other.

The ſharp bone of the ſting-ray was fixed in the lower jaw between two rows of teeth, and at their poſterior part, where the firſt rudiments of the future teeth are formed, and it will be clear to every one, particularly thoſe who are converſant in ſuch matters, that this could not have happened without producing a great deal of pain, ſwelling, and diſorder in the part where it was fixed. It is unneceſſary to enumerate the different kinds of miſchief this might occaſion. Let it ſuffice to obſerve, that on account of the ſpace taken up by this extraneous body, the teeth on each ſide of it, for want of room, could never after be perfectly formed. The teeth on the left-ſide

wanting

wanting their angles to the right, and the teeth on the right-side being deſtitute of their angles to the left.

As it is certain, that the anterior teeth were formerly poſterior ones, and as the teeth in each row were all deficient in one angle, it follows, that they muſt have been formed poſterior to the inſertion of this extraneous body. Again, if we allow that before the accident the animal was in poſſeſſion of perfect teeth, it follows alſo, that they were conſumed and replaced by imperfect ones.

There were ſix teeth in each row, and fifty-two rows, making together about 312 teeth. Now allowing the conſumption to have been equal in all parts of the jaws, it follows, that the animal had already conſumed 312 teeth, and was in poſſeſſion of a like number for future conſumption.

The teeth of ſharks, rays, &c. may be divided into active and paſſive. The active teeth are the anterior ones of each row, ſtanding with their points upwards. The paſſive teeth are the remaining ones, lying one upon another, like the tiles upon a houſe (imbricated), with their points downwards. It appears from the foregoing account, that the anterior or active teeth had been replaced ſix times; and that they might have been renewed ſix times more, making in all twelve times. From which, I think, we may reaſonably conclude, that this does not happen any preciſe number of times; but that the renovation is perpetual during the life of the animal.

The longevity of fiſhes is a fact pretty well eſtabliſhed. In addition to this part of natural knowledge, I have endeavoured to prove, that a part of the inhabitants of the great deep retain, in the article of teeth, a perpetual juvenility, being apparently utter ſtrangers to edentulous old age.

EXPLANATION OF THE PLATES.

Tab. XI. The jaws of the Wolf-fiſh.

A. The palate.

BB. The ſide bones of the upper jaw.

C. The anterior or naſal portion of the ſame.

D. The lower jaw.

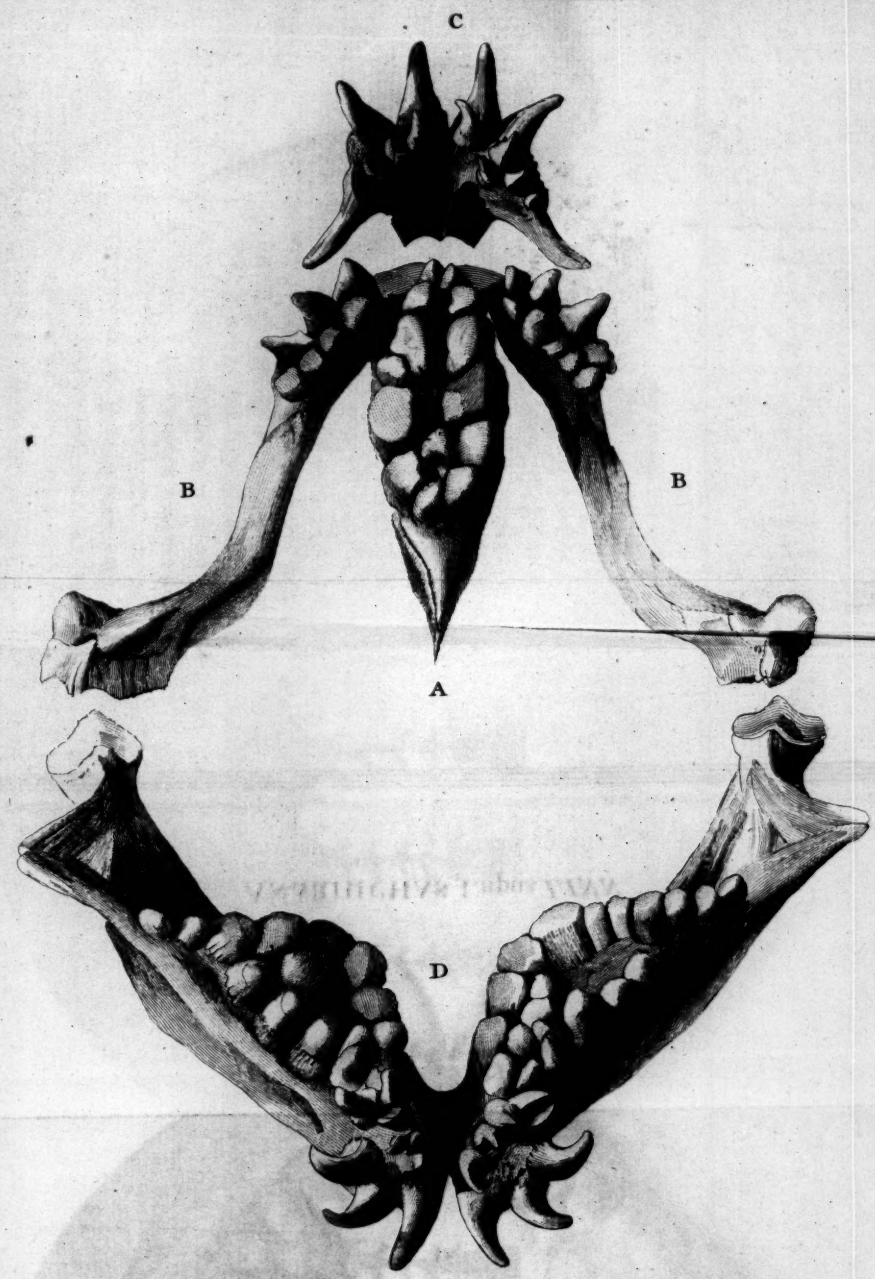
Tab. XII. The teeth of the *Chætodon nigricans* magnified.

Tab. XIII. Part of the lower jaw of a large ſhark.

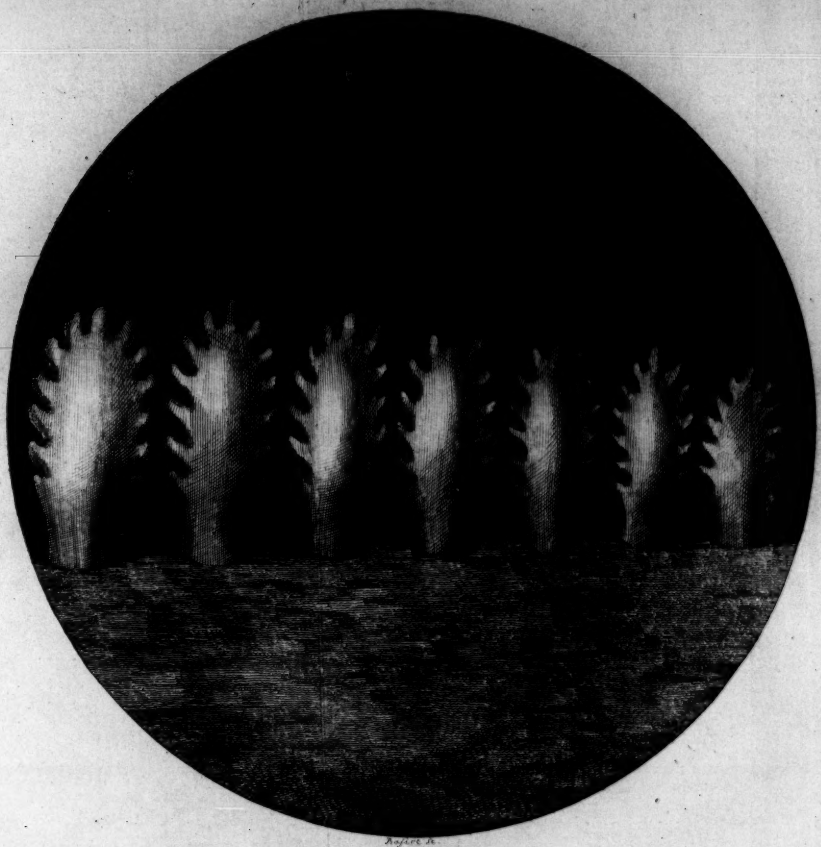
AA. Two rows of perfect teeth.

BB. Two rows of imperfect ones.

C. The bone of the ſting-ray.



ANARHICHAS Lupus *LINN.*



CHÆTODON nigricans *LINN.*

B B
A
A
C

XXI. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1783. *By* Thomas Barker, *Esq.*; *communicated by* Thomas White, *Esq. F. R. S.*

Read March 18, 1784.

		Barometer.			Thermometer.						Rain.		
					In the House.			Abroad.					
		Highest.	Lowest.	Mean.	High.	Low.	Mean	High	Low.	Mean	Lyndon	Selbourn, Hamph.	South Lambeth, Surrey.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.
Jan.	Morn.	29,87	28,38	29,04	47	33½	41	46½	18½	35	1,805	4,43	1,51
	Aftern.				48	35	42	50	27½	40			
Feb.	Morn	30,12	28,08	29,28	48	36	43	49	17½	36½	2,313	5,54	2,98
	Aftern				49½	37	43½	53½	30	43			
Mar.	Morn.	30,01	27,88	29,28	47½	33½	40	44½	21½	33	1,604	2,16	,93
	Aftern.				49½	33½	41½	55	33	43			
Apr.	Morn.	30,14	29,15	29,70	58	46	51	52	32½	43	0,558	,88	,59
	Aftern.				62½	47½	53	67½	41½	55½			
May	Morn.	29,82	29,13	29,48	63½	47	53	55½	34½	44	4,218	2,84	2,36
	Aftern.				66	48½	55	72½	44	56			
June	Morn.	29,85	28,80	29,47	65½	55½	60	63½	46	55	3,033	2,82	4,00
	Aftern.				68½	56½	61½	79	55	67			
July	Morn.	29,89	29,16	29,55	72	61½	66	68	55	61½	2,663	1,45	,78
	Aftern.				75	67	68½	85	67	74			
Aug	Morn.	29,83	29,17	29,49	67	57½	62	67½	46	57	1,102	2,24	2,23
	Aftern.				75	58½	64½	84	57	67			
Sept.	Morn.	29,87	28,77	29,36	62½	54	57½	59	40	50½	1,440	5,53	4,30
	Aftern.				64	56	59	68	55	60			
Oct.	Morn.	29,88	28,99	29,48	60½	46½	53	58	34	44½	0,658	1,71	,72
	Aftern.				61½	48	54	64½	44	54			
Nov.	Morn.	29,96	28,42	29,45	51	42	46½	52½	30½	40	1,783	3,91	1,63
	Aftern.				52	42½	47	54½	37½	45½			
Dec.	Morn.	29,99	28,49	29,29	46	28	40½	43½	8½	32½	1,602	1,10	1,22
	Aftern.				46½	28	40	45½	19	37			
											Inches 22,779	33,71	23,25

The year began with a ſhort dry froſt, then ſhowery, intermixed with froſt. The end of January, and near half of February, ſtormy and wet, and after ten days fine and mild; a ſevere ſeaſon for ſnow, wet, wind, and froſt. The end of February and beginning of March cut the graſs, corn, and ſtock, more than all the winter before. From March 10. to May 27. was a very dry ſeaſon and fine ſeed-time; but ſo dry at laſt the late ſown corn could not come up. The ſpring was pleaſant; but almoſt conſtant froſty mornings till April, and frequent afterward, kept things backward; and though there was ſome fine warm weather the middle of April, yet later in May the drought and N.E. winds ſtopped the growth of things; and two ſharp froſty nights, May 25. and 26. the rime was ſo particularly cutting in the meadows, that the young ſhoots on many oak and aſh trees in the vallies were entirely killed, while thoſe on the hills were unhurt, and ſome of the tops of the trees eſcaped, though the bottoms were blaſted.

May 27. to 30. in a continued three days rain there fell 3¾ inches, which is, I believe, the moſt that has come in one continued unceaſing rain ſince July 1736, when, in about the ſame time, there came five inches; but the rain this May was not alike in all places, for there was not a quarter ſo much in Hampſhire. This rain was of vaſt ſervice to bring up the late ſown corn, and make the graſs grow well; but this and ſome other haſty rains afterwards hurt the meadow graſs, by flooding it three times. Hot weather ſucceeding, it was a very growing time, and ten days together, in the middle of June, were all wet.

During the ſhowery time an uncommon hazineſs began, which was very remarkable all the reſt of the ſummer: the air was all thick both below the clouds and above them, the

hills

hills looked blue, and at a diſtance could not be ſeen; the ſun ſhone very red through the haze, and ſometimes could not be ſeen when near ſetting. There was more or leſs of this haze almoſt conſtantly for a month, and very frequently to the end of the ſummer, and it did not ceaſe till Michaelmas; and neither rain nor fair, wind nor calm, eaſt nor weſt winds, took it away; and it was as extenſive as common, for it was the ſame all over Europe, and even to the top of the Alps. This haze was very like VIRGIL's deſcription of the ſummer after J. CÆSAR's death, which was probably the ſame caſe,

Cum caput obſcurâ nitidum ferrugine texit,

for ruſty iron is a very good deſcription of the colour the ſun ſhone. But by PLUTARCH's account, near the end of C. CÆSAR, that ſummer was very different from this in other reſpects for, he ſays, the ſun gave very little heat, the air was cloudy and heavy, and the fruits not ripened, which was not the caſe this year; for this was a dry haze, the ſummer in general hot and dry, and in ſome countries very much ſo.

I think I never knew more miſchief done by thunder than there was in different places this year, from the beginning of July, and very ſeldom more or hotter weather; yet where they had not thoſe thunder-ſhowers they ſuffered by being burnt up. Here we never wanted graſs after May, and the hay and harveſt were both well got in; but in Surrey, Hampſhire, and Dorſet, they were very much burnt up, had little hay, and as they had a good deal of ſhowery weather in harveſt, their barley ſuffered twice, from not coming up in time, and again in getting it in. As the rain this year was chiefly in ſhowers or ſudden rains, it fell very uncertainly, as appears by comparing

comparing what fell here with that in Hampſhire. The latter part of Auguſt and firſt half of September was ſhowery; but in this country not ſo much as to hurt the harveſt, a great part of which was in firſt. The crop of grain was in general pretty good, but did not yield enough to make up the defect of the laſt year's crop, every body was ſo much out of all ſorts, as the corn laſt year was both ſcarce and bad; grain, therefore, continued dear this year, eſpecially barley.

The ſummer 1782 had been ſo cold and wet, that the flower buds on many trees were very ſmall and not perfected, ſo that this ſpring there was a great want of bloſſoms on the wall fruit and apples, and exceeding few indeed on aſh-trees and hawthorn. I do not know of any aſhkeys at all, nor any bunches of haws, only a few ſcattered ſingle ones; but cherries and plumbs bloſſomed well, and there was no want of fruit; plenty of currants, and vaſt quantities of gooſeberries.

Auguſt 18. a remarkable ball of fire was ſeen between nine and ten at night all over England, and even in foreign countries. It ſeemed to move from north to ſouth or ſouth-eaſt. There was another October 4. but not ſo much obſerved, and ſome ſay another afterward, but little ſeen; but there were very few northern lights this autumn.

The autumn was a very fine one; calm, fair, and mild, but rather too dry for the ſowing of wheat, which, however, in general came up well, and what lay dry was brought up very finely by ten days wet the middle of November; after which it was dry and fine again, an open mild time, with few froſty mornings; but a good deal of dark or miſty weather in December, yet mild till the laſt week, when there came a great ſnow, very ſevere froſt, and cutting ſtrong wind, which ended the year.

XXII. *On the Periods of the Changes of Light in the Star* Algol. *In a Letter from* John Goodricke, *Esq. to the Rev.* Anthony Shepherd, *D. D. F. R. S. Professor of Astronomy at* Cambridge.

Read April 1, 1784.

SIR, York, Dec. 8, 1783.

AS I am now able, by collating some of my late observations on Algol with those I sent you last May, to determine with greater precision the periodical return of its changes, I wish to add this as a kind of supplement to that account.

The method I have here pursued is by taking the intervals between accurate observations of Algol's least brightness or greatest diminution of light made at long distances of time from each other, and dividing those intervals by a certain number of revolutions, as will be best understood by the table below. The reason of my chusing long intervals is, that the number of revolutions being greater, the errors of observation are thereby diminished: all error cannot, however, as yet be excluded, but I think the period is now, by the following calculation, ascertained within ten or fifteen seconds.

Mean times of Algol's least brightness.						d.	h.	′	″
1783	h.	′							
Jan. 14	9	25	an interval of	99	revolutions, each of	2	20	49	14
Oct. 25	6	39							
Jan. 14	9	25	Ditto	106	Ditto	2	20	49	10
Nov. 14	8	17							
Jan. 14	9	25	Ditto	107	Ditto	2	20	49	2
Nov. 17	4	52							
Feb. 6	8	15	Ditto	91	Ditto	2	20	49	3
Oct. 25	6	39							
Feb. 6	8	15	Ditto	98	Ditto	2	20	48	59
Nov. 14	8	17							
Feb. 6	8	15	Ditto	99	Ditto	2	20	48	51
Nov. 17	4	52							
Feb. 26	9	43	Ditto	84	Ditto	2	20	49	14
Oct. 25	6	39							
Feb. 26	9	43	Ditto	91	Ditto	2	20	49	9
Nov. 14	8	17							
Feb. 26	9	43	Ditto	92	Ditto	2	20	49	0
Nov. 17	4	52							
Jan. 31	14	29	Ditto	100	Ditto	2	20	49	4
Nov. 14	8	17							
Mar. 21	8	36	Ditto	84	Ditto	2	20	48	46
Nov. 17	4	52							
Hence the period of Algol's variation is, on a mean,						2	20	49	3

I could have added several more comparisons of the like kind; but these are, I think, sufficient. It is to be remembered, that all the observations contained in the above table are reduced to mean time.

It appears to me now, that the duration of the variation is about eight hours; but, as it is difficult to hit exactly the beginning and end of the variation, this may occasion different observers to differ in this respect. Before I conclude, I beg leave to mention a circumstance deserving of notice; which is, that

that FLAMSTEAD has alſo amongſt other ſtars obſerved Algol, and in two places has marked it of leſs magnitude than at other times, *viz.* of the third magnitude, 1696, January 16. 6 h. 24′, and 1711, December 5. 9 h. 13′, both mean time and old ſtile *. Suſpecting theſe might probably be days of Algol's variation, I computed the interval between them, but could not find a period anſwerable to that which I have above determined. Upon examining more cloſely the obſervations, I find, in that of 1696, he marked at the ſame time the magnitude of ϱ Perſei; which, conſidering eſpecially the nearneſs of ϱ Perſei to Algol, makes this obſervation to be relied on for its juſtneſs, and leſs liable to any miſtake of judgement; whereas the other obſervation of December 5, 1711, is more liable to error or doubtfulneſs, becauſe he did not then mark the magnitude of ϱ Perſei, or of any ſtar of the ſame magnitude near enough to Algol. Preſuming, therefore, on the juſtneſs of FLAMSTEAD's obſervation of 1696, to think that it probably was made at a time when Algol varied, I compared it with one of mine, *viz.* October 25. 6 h. 39′, 1783, and I find there is, in the interval between thoſe obſervations, either 11,176 periods, each of 2 d. 20 h. 49′ 18″; or 11,177, each of 2 d. 20 h. 48′ 56″. The laſt, as it approaches neareſt to the reſults of my beſt obſervations, I think, is the exacteſt determination of the period. This, however, all proceeds upon the ſuppoſition that Algol varied at the time of FLAMSTEAD's obſervation, and alſo that the period is regular.

* Hiſtoria Cœleſtis, vol. II. edit 1725, p. 284. and 534.

The following is a ſhort abſtract of my late obſervations on Algol, when its leaſt magnitude was accurately determined.

Auguſt 17, 1783.

App. time.
h. ′
10 52 About equal to ρ Perſei, though Algol ſeemed to be rather brighter.
11 7 Evidently leſs than ρ Perſei.
11 22 Ditto; but rather difficult to diſtinguiſh them from each other.
11 30 Rather brighter than ρ, and not ſo bright as δ Perſei.
12 0 About the brightneſs of δ Perſei, and rather leſs than β Trianguli.
12 30 Brighter than δ Perſei, and rather not ſo bright as β Trianguli.

From thoſe obſervations, by taking a mean between 11 h. 7′ and 11 h. 22′, it appears, that its leaſt brightneſs happened at 11 h. 14′; true, I think, to 5′.

October 25.

6 40 It was conſiderably leſs than ρ Perſei.
7 5 Ditto.
7 20 Equal to ρ Perſei, though Algol ſeemed rather leſs.
7 35 About equal to ρ Perſei.
7 50 Brighter than ρ, and alſo than δ Perſei.
8 25 About the third magnitude, and equal to β Trianguli.
9 35 Between the ſecond and third magnitude; brighter than β Arietis, and rather leſs than α Pegaſi.
10 10 About the ſecond magnitude; rather brighter than α Pegaſi, rather leſs than β Caſſiopeæ, and not ſo bright as α and γ Caſſiopeæ.

Rather

App. time.
h. ′
10 40 Rather brighter than β Caſſiopeæ, but leſs than α and γ.
11 0 Nearly equal to, if not rather brighter than, γ Caſſiopeæ, and leſs than α Caſſiopeæ*.

In 20′ afterwards it was of the ſame brightneſs; hence we may conclude, that the variation has ended at 11 h. 0′.

Its leaſt brightneſs from the obſervations appears to have happened at 6 h. 55′; true, I think, to 10′.

November 11.

10 5 Third magnitude; not much different from ε Perſei and β Trianguli.
10 45 Between the third and fourth magnitude; believe equal to δ Perſei.
11 14 Leſs than ρ Perſei.
11 48 Ditto; but think it rather increaſed.

Its leaſt brightneſs from thoſe obſervations appears to have happened at 11 h. 31′; true, I believe, to a quarter of an hour. The weather was rather hazy.

November 14.

5 0 Between the ſecond and third magnitude, and leſs than β Caſſiopeæ.
5 45 A little brighter than β Arietis.
6 50 Not ſo bright as β Arietis, and rather brighter than β Trianguli.
8 10 A little brighter than ρ Perſei, and believe equal to δ Perſei.

* Algol's uſual and greateſt brightneſs, by my later and more accurate obſervations, is thus: a little leſs than α Caſſiopeæ, brighter than β Caſſiopeæ and α Pegaſi, and rather a little brighter than γ Caſſiopeæ.

App. time.
h. ′
8 25 Leſs than ρ Perſei.
8 40 Ditto.
9 0 Equal to ρ, though Algol appeared rather brighter.
9 15 A little brighter than δ and ρ Perſei.

By taking a mean between 8 h. 25′ and 8 h 40′, it appears, its leaſt brightneſs happened at 8 h. 32′; true to 10 minutes. The weather was rather hazy during ſome part of this obſervation.

November 17.

4 58 A little leſs than ρ Perſei.
5 15 Ditto.
5 35 Rather brighter than ρ Perſei.
5 50 A little brighter than ρ Perſei, but leſs than δ Perſei.
6 5 Rather brighter than δ Perſei.
6 40 Equal to β Trianguli, and brighter than ε and ζ Perſei.
7 20 A little brighter than β Arietis.
8 30 Between the ſecond and third magnitude, and equal to β Caſſiopeæ, but leſs than α and γ.
8 50 Second magnitude, and equal to γ Caſſiopeæ.
9 25 Nearly the ſame, if not rather brighter.

The variation has therefore ended at 9 h. 0′ nearly, and its leaſt brightneſs by taking a mean between 4 h. 58′ and 5 h. 15′, happened at 5 h. 7′; true, I believe, to 10 minutes. The weather was fine.

I have ſeveral more obſervations on Algol, where I have not been able to aſcertain its leaſt brightneſs, which all happened agreeable to the period as above determined; *viz.* May 20. July 5. and 22. Auguſt 14. September 6. 9. 12. and 26. October 2. 5. 19. and 22. and December 7.

XXIII. *Experiments and Obſervations on the* Terra Ponderoſa, &c. *By* William Withering, *M. D.; communicated by* Richard Kirwan, *Eſq. F. R. S.*

Read April 22, 1784.

SECTION I.

Terra ponderoſa aërata.

THIS ſubſtance was got out of a lead-mine at Alſton-Moor, in Cumberland. I firſt ſaw it in the valuable collection of my worthy and ingenious friend MATTHEW BOULTON, Eſq. at Soho; who, when he picked it up, conjectured from its weight that it contained ſomething metallic. About two years ago I ſaw it in his poſſeſſion; and partly from its appearance, being different from that of any calcareous ſpar I had ſeen, and partly from its great weight, I ſuſpected it to be the ſpatum ponderoſum.

A few experiments made at the moment confirmed my ſuſpicions, at leaſt ſo far as to ſhew that it contained a large proportion of the terra ponderoſa united to fixed air; but I did not then flatter myſelf that it would prove ſo pure as I afterwards found it to be.

Profeſſor

Professor BERGMAN, in his Sciagraphia Regni Mineralis, published last year at Leipsic, conjectures (§ 58.), with his usual sagacity, "Terra ponderosa *nitrata* forte alicubi nativa "occurrit, a nemine tamen adhuc inventa, quod etiam valet "de *aërata*."

I was much delighted by the detection of a substance which promises to be of very considerable utility in chemical inquiries, and more so when I found it to be a native of this country; for it is not improbable, that it may be met with in many other mines, besides that at Alston-Moor.

Mr. BOULTON, with his usual benevolence, presented me with a piece of it, part of which accompanies this paper, for the inspection of the Members of the Royal Society.

More obvious Properties.

Its general appearance is not much unlike that of a lump of alum; but, upon closer inspection, it seems to be composed of slender spiculæ in close contact, but more or less diverging. It may be cut with a knife. Its specific gravity is from 4,300 to 4,338.

It effervesces with acids, and melts under the blow-pipe, though not very readily. Placed in a covered crucible, in a hot parlour fire, it lost its transparency.

After exposure to a moderate heat in a melting furnace, it adhered to the crucible, and exhibited signs of fusion; but was not diminished in weight, did not feel caustic when applied to the tongue, nor had it lost its property of effervescing with acids.

Hence

Hence it is probable, that its loss of transparency was rather occasioned by numerous small cracks, than by any escape of the water of crystallization, or of its aërial acid.

EXPERIMENTS.

A. 500 grains, dissolved in muriatic acid, in such a manner that nothing but elastic fluid could escape, lost in solution 104 grains, and there remained an insoluble residuum of nearly 3 grains.

2. In another experiment 100 grains lost in solution 21 grains, and there remained 0,6 of a grain of insoluble matter.

B. 100 grains dissolving in dilute muriatic acid, gave out 25 ounce measures of air. This air was received in quicksilver, and when the spar was wholly dissolved, the solution was boiled, in order to drive out what air might be lodged in it.

2. This air was heavier than atmospheric air; it was readily absorbed by agitation in water, it precipitated lime from lime-water, and it extinguished flame. The water which had absorbed it changed the blue colour of litmus slowly * to a red; so that this elastic fluid was undoubtedly fixed air.

C. The solution (B) by the addition of mild fossil fixed alkaly, afforded a precipitate which, after proper washing and drying, weighed 100 grains.

2. This precipitate, upon being again dissolved in marine acid, yielded only 20 ounce measures of fixed air.

* Other acids turn the blue of litmus instantly to a red, whilst water, impregnated with fixed air, does not change the litmus immediately; but, after some seconds, the red colour begins to appear, and then gradually grows more distinct.

D. To a ſaturated ſolution in marine acid mild fixed vegetable alkaly was added; the earth was precipitated, and a quantity of fixed air eſcaped.

2. The ſame thing happened when mild foſſil alkaly was added.

3. When cauſtic vegetable alkaly was uſed, the precipitation took place, but without any appearance of effervescence.

4. 50 parts diſſolved in marine acid loſt, during the ſolution, nearly 10,5. This ſolution, upon the addition of cauſtic vegetable alkaly, let fall a precipitate which, when waſhed and dried, weighed 45,5.

5. Phlogiſticated alkaly precipitated the whole of the earth from part of the ſolution D; for mild fixed alkaly afterwards added to the filtered liquor occaſioned no further precipitation.

E. Part of the precipitates D. 1. 2. after expoſure to a ſtrong heat in a crucible, was thrown into water. Next morning the water was completely covered with an ice-like cruſt, and had the acrid taſte of lime-water in a very high degree.

2. The ſmalleſt portion of vitriolic acid added to this water occaſioned an immediate and copious precipitation; and when this acrid water was diluted with 200 times its bulk of pure water, the precipitation upon the addition of vitriolic acid was yet ſufficiently obvious.

3. A ſingle drop of this acrid water, added to ſolutions of tartar of vitriol, GLAUBER's ſalt, vitriolic ammoniac, alum, Epſom ſalt, ſelenite, occaſioned an immediate precipitation in all of them.

F. The precipitate thrown down by the cauſtic vegetable alkaly (D. 3.) was put into water, in expectation that it would make lime-water; but neither upon ſtanding, nor after boiling, did this water exhibit any precipitation when concentrated vitriolic

vitriolic acid was dropped in it; nor had it any acrimonious or other peculiar taſte.

G. Concentrated vitriolic acid was added to one portion of the precipitate D. 3.; concentrated nitrous acid to a ſecond portion; and marine acid to a third portion. No effervescence could be obſerved, nor was there any appearance of ſolution. After ſtanding one hour water was added; and the acids, thus diluted, were ſuffered to remain upon the portions of the precipitate for another hour. They were then decanted, and ſaturated with mild foſſil fixed alkaly, but without any appearance of precipitation.

H. The part precipitated by the phlogiſticated alkaly, when mixed with nitre and borax, and fluxed by a blow-pipe upon charcoal, formed a black glaſs; upon flint-glaſs, a white; and upon a tobacco-pipe an opaque yellowiſh white one.

2. Another portion melted with ſoap and borax in a crucible, formed a black glaſs, but without any metallic appearance.

I. The inſoluble reſiduum (A.) was boiled in water, the water decanted, and mild fixed alkaly added, but without any ſubſequent precipitation.

2. This inſoluble powder was not attacked by the nitrous or marine acids; but being put into vitriolic acid, and boiled a conſiderable time until the acid became highly concentrated, it diſſolved entirely, and ſeparated again upon the addition of water. It will appear in the ſequel, that the ſame thing happens to marmor metallicum, when diſſolved by boiling in the acid of vitriol.

CONCLUSIONS.

Hence it appears, that 100 parts of this ſpar contain

Terra ponderoſa pura -	78,6
Marmor metallicum -	,6
Fixed air - - -	20,8
	100

OBSERVATIONS.

1ſt, The quantity of mild fixed alkaly neceſſary to ſaturate an acid, previouſly united to the terra ponderoſa, contains more fixed air than is neceſſary to ſaturate that quantity of terra ponderoſa D. 1. 2.

2dly, The terra ponderoſa, when precipitated from an acid by means of a mild fixed alkaly (D. 1. 2.), readily burns to lime; and this lime-water proves a very nice teſt of the preſence of vitriolic acid. E. 2. 3.

3dly, It is very remarkable, that the terra ponderoſa ſpar, in its native ſtate, will not burn to lime. In the lower degrees of heat it ſuffers no change, as was before obſerved, beſides the loſs of its tranſparency. When urged with a ſtronger fire, it melts and unites to the crucible, but does not become cauſtic.

I buried it in charcoal-duſt in a covered crucible, and then expoſed it to a pretty ſtrong heat; but it did not part with its air.

May we not conjecture, then, that as cauſtic lime cannot unite to fixed air without the intervention of moiſture, and as this ſpar ſeems to contain no water in its compoſition, that it

is

is the want of water which prevents the fixed air assuming its elastic aërial state? This supposition becomes still more probable, if we observe that when the solution of the spar in an acid is precipitated by a mild alkaly, C. 1. 2. some water enters into the composition of the precipitate, for it weighs the same as before it was dissolved, and yet contains only 20 ounce measures of fixed air, whilst the native spar contained 25 ounce measures; so that there is an addition of weight equal to that of 5 ounce measures of fixable air, or 31½ grains to be accounted for, which can only arise from the water; and this precipitate, thus united to water, readily loses its aërial acid in the fire, E. 1.

4thly, Professor BERGMAN supposes the terra ponderosa to be a metallic earth *; its entire separation from an acid by means of the phlogisticated alkaly (D. 5.) certainly favours such a supposition; but, if it be so, it is evident from experiments H. 1. 2. that other means than those commonly employed must be used to effect its reduction.

5thly, The precipitate made by the caustic vegetable alkaly D. 4. taking some of the alkaly down with it, and thus forming a substance neither soluble in water nor in acids, is a very curious phænomenon.

I afterwards varied the experiment by adding the terra ponderosa lime-water (E.) to caustic vegetable and caustic fossil alkaly. In both cases this insoluble compound was immediately formed; but not so when caustic volatile alkaly was used. This composition of an alkaly and an earth certainly deserves more attention than I am at present able to bestow upon it.

6thly, As it appears from experiments D. 1. 2. 3. 4. that

* See preface to his Sciagraphia Regni Mineralis.

fixed alkalies, both mild and caustic, separated the terra ponderosa from marine acid, I was at a loss to know why Professor BERGMAN, in his admirable table of simple elective attractions, placed the terra ponderosa, caustick immediately under the vitriolic, nitrous, and marine acids, and consequently above the caustic alkalies. I was interested in the reality of the facts, because I had so seldom seen reason to doubt the observations of that very excellent chemist, and therefore made the following experiments.

To different portions of terra ponderosa salita and terra ponderosa nitrata I added, drop by drop, caustic vegetable, caustic fossil, and caustic volatile alkalies. In every case the EARTH was thrown down; and I have so often repeated these experiments to satisfy myself and others, that I am persuaded the terra ponderosa caustica ought to be placed below the alkalies, except in the column appropriated to the vitriolic acid; and it may be separated even from that acid, by the vegetable fixed alkaly, if the alkaly be applied *via ficca*, as will appear in the next section.

7thly, The necessity for using pure acids upon many occasions, and the difficulty of obtaining them pure, are sufficiently obvious. The VITRIOLIC ACID, as made in the large way, is generally pure enough for most purposes. It is apt to get coloured by inflammable matter; but this is seldom an inconvenience; and, when it would be so, it is easy to drive it off by boiling the acid in a Florence flask over a common fire. But there is another cause of impurity in this acid, which appears upon diluting it with water; for then it becomes milky, and in a short time a powder subsides.*

The

* About two years ago I examined this powdery matter; both that which was thrown

The acid may be freed from this powder either by distillation in glass vessels, which is a tedious and dangerous process, or by the affusion of water; and, after the powder has subsided, a gentle evaporation will drive off most of the superfluous water.

NITROUS ACID may be freed from vitriolic and marine acids, by solution of silver in the acid of nitre, as is daily practised: but the MARINE ACID has not, to my knowledge, been purified by any other method than the laborious one of re-distilling it from common salt. It is generally mixed with vitriolic acid, and often in large proportion. There is no temptation, and scarcely an opportunity, for it to be contaminated by nitrous acid. From the vitriolic acid then it may be readily purified by the addition of terra ponderosa caustica dissolved in water, or by the terra ponderosa salita. If the latter be used, a small

thrown down by dilution with water, and also some which Dr. PRIESTLEY gave me, being the residuum of vitriolic acid distilled to dryness in a flint-glass retort.

1st, Repeated boiling in water, reduced 61 grains to 2 grains.

2dly, This solution, by gentle evaporation, afforded 3 grains of crystals, as hard and as tasteless as selenite.

3dly, To these crystals, re-dissolved in water, mild fossil alkaly was added, and a white powder precipitated.

4thly, This powder, after exposure to a pretty sharp heat, was thrown into water; part of it dissolved, and the water got the taste and other properties of lime-water.

5thly, The insoluble part (1.) suffered no change by boiling in nitrous acid; one-half of it mixed with borax, and exposed to the blow-pipe upon charcoal, vitrified; the other half, mixed with borax and charcoal-dust, likewise vitrified.

CONCLUSIONS. It appears, then, that the greater part of this substance was calx vitriolata, or selenite; the remainder a vitrifiable earth.

I had before found, that the terra ponderosa vitriolata, too heavy gypsum, would dissolve in concentrated vitriolic acid; but always separated in a powdery form upon the affusion of water; and now it appears, that calx vitriolata, or selenite, does the same.

portion

portion of the acid muſt firſt be tried in a diluted ſtate, from whence we muſt judge how much of the terra ponderoſa ſalita the whole will require; or elſe the whole of the acid muſt be diluted with water. Whether we uſe the terra ponderoſa diſſolved in water or in marine acid; in either caſe the acid of vitriol immediately ſeizes upon it, and ſubſides with it in form of an inſoluble powder.

As there are reaſons for preferring the marine acid in ſeveral of the nicer and more important enquiries of chemiſtry, this ready method of purifying it cannot but prove acceptable.

SECTION II.

Terra ponderoſa vitriolata. BERGMAN's Sciagraphia, §§ 58. 89.

Variety. Heavy Gypſum. Ponderous Spar.

Marmor Metallicum. CRONSTEDT Min. § 18. 2. 19. C.

From Kilpatrick-hills near Glaſgow. A ſort, with ſmaller cryſtals, amongſt the iron ore about Ketley in Shropſhire. In the lead mines at Alſton-Moor.

More obvious Properties.

White; nearly tranſparent, but has not the property of double refraction; compoſed of laminæ of rhomboidal cryſtals; decrepitates in the fire. Specific gravity from 4,402 to 4,440.

EXPE-

EXPERIMENTS.

A. 100 grains exposed to a red heat for one hour, in a black lead crucible, lost five grains in weight; but as a sulphureous smell was perceptible, I suspected that a decomposition had taken place, and therefore exposed another portion to a similar heat for the same space of time in a tobacco-pipe. This had no smell of sulphur, nor was it diminished in weight.

2. It is barely fusible under the blow-pipe; but with borax fluxes readily into a white opake glass.

B. 100 grains, ground in a mortar, and washed over extremely fine by repeated additions of water, were boiled in the same water, and, after settling, the water was poured off. The powder, when dried, had not sensibly lost weight.

2. To separate portions of the washing water, were added mild vegetable and mild fossil alkaly; but without any appearance of precipitation. Nitre of mercury gave a very slight brownish cloud, barely discernible; and nitre of silver an extremely slight bluish appearance.

3. The same powder, boiled again in fresh water, did not affect the water at all; for it stood the test of nitre of silver without any change.

C. Portions of the powder B. were boiled in vitriolic, nitrous, and muriatic acids, of the usual strength, for several minutes. The acids were then saturated with vegetable fixed alkaly, but without any appearance of precipitation, nor had the portions of powder lost any weight.

2. But when boiled in vitriolic acid, until that acid became very much concentrated and nearly red-hot, the whole of it dissolved; but, separated again upon the addition of water, was

not altered in its weight, was not acted upon by acids of the uſual ſtrength, and had, under the blow-pipe, the properties mentioned at A. 2.

3. Some of the ſolution in the concentrated vitriolic acid was left expoſed to the atmoſphere, that the acid might ſlowly attract water. After ſome days, beautiful cryſtals appeared in the ſhapes of ſtars, faſciæ, and other radiated forms.

4. To another portion of this ſolution mild fixed vegetable alkaly was added; but the precipitate appeared to be the marmor metallicum unchanged.

D. One ounce of this marmor metallicum in fine powder was fluxed in a crucible with two ounces of ſalt of tartar, until it ran thin. This ſubſtance, boiled with water in a Florence flaſk, left a reſiduum of ſix drams.

E. This reſiduum was thrown into water, and pure nitrous acid added, until there was no more effervescence. The undiſſolved part weighed 52 grains.

F. This undiſſolved part appeared to be the original ſubſtance no ways changed; for it did not diſſolve in nitrous or marine acids, but did wholly diſſolve in the greatly concentrated and boiling vitriolic acid, from which it was again ſeparated by the addition of water. (C. 2.)

G. The ſolution D. was ſaturated with diſtilled vinegar, and then evaporated to dryneſs, but with leſs than a boiling heat. The ſal diureticus, thus formed, was waſhed away with alcohol. The remaining ſalt weighed 5 drams nearly.

2. This ſalt had the appearance and the taſte of vitriolated tartar; it decrepitated in the fire; roaſted with charcoal-duſt, it formed a hepar ſulphuris; and with muria calcarea gave a precipitation of ſelenite.

H. The salt, formed with the nitrous acid (E), shot readily into beautiful permanent crystals, of a rough bitterish taste.

2. Some of this salt was deflagrated with nitre and charcoal, and the alkaly afterwards washed away.

3. The residuum, being the earth of the marmor metallicum, was very white, burnt to lime, and again formed an insoluble compound with acid of vitriol.

I. 100 grains of terra ponderosa aërata were dissolved in diluted marine acid. Vitriolic acid was dropped into this solution, until no more precipitation ensued. The precipitate, after very careful washing and drying, was exposed to a red heat in a covered tobacco-pipe for half an hour: when cool, it weighed 117 grains.

2. 50 grains of terra ponderosa aërata in a lump were put into diluted vitriolic acid; but the action of the acid upon it was hardly sensible, even when made hot.

Marine acid was then added, and after some time an effervescence appeared. The terra ponderosa vitriolata, thus formed, after proper washing and drying, was exposed to a red heat for an hour: it then weighed 58,4 grains.

CONCLUSIONS.

1st, It appears that the marmor metallicum is composed of vitriolic acid and terra ponderosa, D. E. F. G. H.

2dly, That this compound, though probably soluble in water, has so little solubility as almost to escape detection by the nicest chemical tests, B. 1. 2. 3.

3dly, That it is not soluble in acids of the usual strength; but that it perfectly and entirely dissolves in highly concen-

trated vitriolic acid, from which it again separates entire and unchanged upon the affusion of water, C. 1. 2.

4thly, That it cannot be decomposed (*via humida*) by mild fixed alkaly, C. 4.

5thly, That it may be decomposed (*via sicca*) by the vegetable fixed alkaly, D. E. G. H.

6thly, That it may be decomposed by inflammable matter, uniting to its acid, and forming sulphur; but that it cannot be decomposed by heat alone, A. 1.

7thly, From experiments I. 1. 2. it appears, that 100 parts of this substance contain

Vitriolic acid pure	32,8
Terra ponderosa pure	67,2
	100

For the 100 parts of terra ponderosa aërata made use of in the experiment I. 1. would lose during the solution 20,8 of fixed air (§ 1st, A.); then, deducting 0,6 for the marmor metallicum contained in the terra ponderosa aërata (§ 1st. A. 1. 2.), there remains 78,6 of pure terra ponderosa. This, when saturated with vitriolic acid, and made perfectly dry, weighed 117; consequently it had taken 38,4 of vitriolic acid.

OBSERVATIONS.

The apparent insolubility of terra ponderosa aërata in the diluted vitriolic acid (I. 2.) can be accounted for by remarking, that the moment the surface of the lump was acted upon by the acid, an insoluble coat of marmor metallicum was formed upon it, which effectually precluded any further operation of the acid.

Professor

Professor BERGMAN, in order to obtain the earth from the terra ponderosa vitriolata, directs the latter to be roasted with fixed alkaly, and the dust of charcoal; but I have always done it by charcoal dust alone, though probably this method may require a greater degree of heat.

It has been remarked, that terra ponderosa and calx of lead resemble each other in many respects; and I must add, that the vitriol of lead dissolves in the highly concentrated vitriolic acid much in the same manner that the marmor metallicum does, and like this too separates upon the affusion of water; but I never observed it disposed to crystallize.

The marmor metallicum may probably be useful in some cases where a powerful flux is wanted; for I once mixed some of it with the black flux, and exposing it to a pretty sharp heat, it entirely ran through the crucible. May not, therefore, some of the more common varieties of it be used advantageously as a flux to some of the more refractory metallic ores?

SECTION III.

Terra ponderosa Vitriolata.

Variety, Calk or Cauk.

Marmor Metallicum, CRONSTEDT Min. § 18. B?

Plentiful in the Mines in Derbyshire.

More obvious Properties.

Of a white or reddish colour; crystallized in rhomboidal laminæ, but these very much intermixed and confused. Loses

little or nothing of its weight by being made red-hot. Specific gravity 4,330.

EXPERIMENTS.

A. Ground in a mortar, and waſhed over, the waſhing water, when decanted, gave no precipitation with mild vegetable alkaly; but with nitre of ſilver and nitre of mercury the ſlighteſt cloud imaginable.

B. 100 grains boiled in marine acid weighed, after proper waſhing and drying, 99,5.

C. The acid ſolution B let fall a Pruſſian blue upon the addition of a ſingle drop of phlogiſticated fixed alkaly; and, when ſaturated with mild foſſil alkaly, afforded an ochry-coloured precipitate.

D. This precipitate, collected and waſhed, weighed half a grain. It was roaſted with tallow, and then was wholly attracted by a magnet.

E. A quantity of the cauk, finely powdered, was mixed with charcoal-duſt, and roaſted in a crucible at a white heat, for five hours, freſh charcoal-duſt being occaſionally added. It gave out a ſtrong ſmell of ſulphur.

F. To this roaſted cauk nitrous acid was added, which diſſolved the greater part of it; producing, during the ſolution, ſome effervescence, and a ſtrong ſmell of hepar ſulphuris.

G. Some of this ſolution, after proper evaporation, afforded beautiful cryſtals, not deliqueſcent, exactly reſembling thoſe obtained from the marmor metallicum, (§ II. H.).

H. To other portions of the ſolution F, were added fixed vegetable and foſſil alkalies, and likewiſe volatile alkaly, each of which precipitated the earth from the acid.

I. This

I. This earth, after exposure to a white heat for one hour, became caustic, and made lime-water, similar in properties to to that mentioned at § Ist. E.

K. Some of the part not acted upon by the nitrous acid F, dissolved entirely by boiling in highly concentrated vitriolic acid, and wholly separated again by the affusion of water. More water was added, and the whole was boiled again; but the filtered liquor gave no signs of precipitation upon the most liberal addition of mild fixed vegetable alkaly.

CONCLUSIONS.

It appears, therefore, that 100 parts of Derbyshire cauk contain

Marmor metallicum -	99,5
Calciform iron - -	,5
	100

And it is probable, that the redder pieces contain a little more iron.

SECTION IV.

Terra ponderosa vitriolata.

Variety, radiated Cauk.

Gypsum crystallisatum capillare. CRONSTEDT Min. § 49. B.

From Pennely by the Bog, near Minsterley, in Shropshire, fifteen miles from Salop, on the road to Montgomery.

More

More obvious Properties.

Somewhat gloſſy like ſatin; yellowiſh-white, opake; compoſed of ſlender ſpiculæ ſet cloſe together, and pointing from a center.

In ſome pieces there are concentric circles of a ſemi-tranſparent horn-like appearance. It is not very brittle; may be ſhaved with a knife; loſes little or nothing of its weight by being made red-hot. Its ſpecific gravity 4,000; but after ſoaking one night in the water 4,200, or more.

EXPERIMENTS.

When treated in the ſame manner that the Derbyſhire cauk was, in the preceding ſection, 100 parts of it appeared to contain

Marmor metallicum -	97,7
Calciform iron - -	2,3
	100

Suſpecting that the preſence of ſo ſmall a proportion of iron could hardly occaſion the whole of the apparent differences betwixt the Shropſhire and Derbyſhire cauks and the marmor metallicum; and thinking it not improbable, that they might contain lead; I mixed ſome of them with charcoal-duſt and borax, but could not by means of the blow-pipe produce any metallic appearance, although vitriol of lead, treated in the ſame manner, was readily reduced.

I then mixed four parts of cauk with one part of vitriol of lead; the lead could ſtill be reduced, though not ſo readily as before.

GENERAL

GENERAL OBSERVATIONS.

The terra ponderosa seems to claim a place betwixt the earths and the metallic calces. Like the former, it cannot be made to assume a metallic form; but, like the latter, it may be precipitated from an acid, by means of phlogisticated alkaly. In many of its properties it much resembles the calx of lead; and in others, the common calcareous earth, but still seems sufficiently different from that to constitute a new genus, as will appear from a little attention to the following circumstances.

Terra ponderosa,	Terra calcarea,
When dissolved in water, precipitates upon the addition of the smallest portion of vitriolic acid.	Dissolved in water, does not precipitate upon the addition of vitriolic acid.
Its gypsum, therefore, is insoluble.	Its gypsum, therefore, is soluble.
With the nitrous and marine acid, forms crystals which do not deliquesce.	With nitrous and marine acids forms salts so deliquescent that they cannot be kept in a crystallized form.
Decomposes vitriolic salts *via humida*.	Does not decompose vitriolic salts.

It has been called terra ponderosa, or heavy earth, upon account of the great specific gravity of its gypsum; its spar is likewise heavy enough to countenance such an appellation; but the earth itself does not appear to be a heavy substance, and I imagine the great weight of its compounds with the vitriolic and aërial acids is owing to the absence of water.

Birmingham, Nov. 1783.

XXIV. *Observations du Paſſage de* Mercure *ſur le Diſque du Soleil le* 12 *Novembre*, 1782, *faites à l'Obſervatoire Royal de* Paris, *avec des réflexions ſur un effet qui ſe fait ſentir dans ces mêmes Obſervations ſemblable à celui d'une Réfraction dans l'Atmoſphere de* Mercure. *Par* Johann Wilhelm Wallot, *Membre de l'Académie Electorale de Sciences et Belles Lettres de* Manheim, *&c. Communicated by* Joſeph Planta, *Eſq. Sec. R. S.*

Read April 29, 1784.

1. LES paſſages de Mercure ſur le diſque du ſoleil ſont d'autant plus intéreſſans pour les aſtronomes, qu'ils donnent principalement le moyen de déterminer avec plus d'exactitude la poſition des nœuds de ſon orbite, et que la difficulté de voir cette planète dans ſes autres aſpects avec le ſoleil en rend les obſervations plus précieuſes.

2. Deux circonſtances aſſez déſavantageuſes qui devaient accompagner particulierement le paſſage dont il s'agit ici, ſavoir la proximité du ſoleil de l'horizon, et Mercure paſſant trop près du bord de cet aſtre, ſemblaient par leur nature offrir trop d'inconvéniens pour en eſpérer des obſervations bien exactes; cependant l'encouragement qu'a donné le beau tems qu'il fit toute la journée du 12 Novembre, nous ayant fait apporter une plus grande attention aux obſervations, nous autoriſe maintenant à en avoir une meilleure opinion. Je crois pouvoir aſſurer ſans oſtentation d'y avoir réuſſi aſſez pour être ſatisfait des miennes, et pour oſer les garantir autant que la nature des

choses peut le permettre. Si je puis me flatter d'avoir obtenu de ce passage une observation très exacte et peut-être la plus complette, je ne dissimulerai pas que je dois en grande partie cet avantage à M. DE CASSINI qui, m'ayant laissé la meilleure lunette * qu'il y ait à l'Observatoire Royal, m'avait mis par là dans le cas d'employer la plus grande vigilance pour mériter par l'exactitude de mes opérations la confiance qu'on me témoignait dans une occasion aussi importante.

3. Nous avons fait (M. DE CASSINI et moi) toutes les observations nécessaires pour constater avec la plus grande exactitude l'état de notre pendule; et, en réduisant mes observations au tems vrai, je n'ai pas même négligé les dixièmes de seconde. Cette précision scrupuleuse paraîtra peut-être superflue dans de pareilles observations, mais on verra par la suite de ce Mémoire les raisons qui m'y ont déterminé. Voici mes observations dans le même ordre où elles se sont faites, et réduites au tems vrai de la méridienne de l'Observatoire Royal de Paris.

	Tems vrai. h. ′ ″	
Entrée	à 2 56 28,8	Je soupçonne la planète. Contact extérieur de l'entrée.
	à 2 58 28,8	J'estime Mercure entré à moitié. Centre de ☿ sur le bord du ☉.
	à 3 2 3,8	Contact intérieur de l'entrée.
	à 3 3 45,8	Mercure absolument détaché du soleil.

En mesurant le diamètre de Mercure sur le disque du soleil je l'ai trouvé par deux fois exactement de la même quantité, savoir de 9 parties du micromètre objectif, qui valent 9″,535 de degrés du grand cercle.

Sortie	à 4 17 18,4	Contact intérieur de la sortie.
	à 4 20 36,4	Le centre de Mercure sur le bord du soleil.
	à 4 22 53,4	Contact extérieur de la sortie. Mercure totalement perdu de vue.

* Une excellente lunette achromatique de DOLLOND de 3 pieds.

Le bord du soleil était si ondoyant que Mercure, aux approches de sa sortie totale, ressemblait exactement à un corps flottant sur les vagues d'une eau fortement agitée, et qui tantôt disparait entièrement, tantôt élevé par les vagues se montre en partie et quelquefois tout entier. Ces vagues ou ondulations allaient toujours dans le même sens du N. Ouest au Sud Est. Leur mouvement était assez rapide, et c'est précisément la rapidité de ce mouvement qui m'a singulièrement favorisé l'observation du contact extérieur de la sortie de Mercure, parceque je ne la perdais jamais de vue qu'un instant.

Je terminerai le détail de mes observations par assurer, que je n'ai pas apperçu la moindre apparence d'une atmosphère ou nébulosité autour de Mercure pendant toute la durée de son passage, quoique la lunette me représentât tous les objets très distinctement. J'ai toujours vu le disque de Mercure bien noir, et également bien terminé dans toute sa circonférence qui me paraissait toujours tranchée nette, surtout dans le commencement où les ondulations étaient moins fortes jusques vers le milieu du passage. Mais cela ne m'empêchera pas d'être très persuadé de l'existence d'une atmosphère autour de Mercure, comme autour de tous les corps célestes, et qu'on peut fort bien l'avoir apperçue dans ce passage sous un ciel plus pur et plus beau que celui de Paris.

Résultats du calcul des observations précédentes selon leurs différentes combinaisons.

4. La methode que j'ai suivie pour réduire les observations de ce passage au centre de la terre, m'est en quelque sorte particulière; mais comme elle n'est pas entièrement nouvelle puisqu'elle

qu'elle ne diffère de toutes les methodes connues qu'en ce que je l'ai ſimplifiée en la rendant abſolument directe, je me contenterai d'en donner une idée générale. Je n'ai employé dans mes calculs que ce qui eſt donné directement par obſervation, ou bien des quantités plus exactement données par les tables, telles que le diamètre du ſoleil, ſon mouvement horaire et celui de Mercure. Mais ce qui caractériſe eſſentiellement cette méthode, c'eſt qu'en combinant les obſervations toujours enſemble deux à deux, on a la durée ou le tems écoulé d'une obſervation à l'autre qui eſt une des principales données du problême, et la plus exacte qu'on puiſſe ſe procurer par obſervation. Or, quand l'obſervation nous fournit directement des données exactes, je ne vois abſolument pas la néceſſité d'en aller chercher de moins exactes pour les faire entrer dans le calcul. C'eſt pourtant ce que font quelques aſtronomes modernes *, qui, en recommendant dans leur Traité d'Aſtronomie de calculer les obſervations ſéparément afin, diſent-ils, de multiplier les réſultats et d'en déduire plus exactement par un milieu la quantité qu'on cherche, ſont obligés pour cet effet de ſuppoſer à peu près connu le milieu du paſſage et la plus courte diſtance des centres †. Ce raiſonnement, auſſi éloigné des principes de la géométrie, que des regles de l'analyſe, me paraît encore illuſoire quant à l'exactitude qu'on eſpère obtenir de la multiplicité des réſultats ainſi déterminés; voici pourquoi.

Je ſuppoſe pour un inſtant qu'on prenne au hazard deux obſervations, et qu'on les calcule ſéparément chacun ſuivant ce précepte; il eſt certain que ſi l'on ne ſuppoſe pas le milieu du paſſage et la plus courte diſtance des centres tels que les don-

* Principalement M. De la Lande dans ſon Traité d'Aſtronomie, édition de 1771, livre XI. art. 2152.

† Ibid. art. 2062 et 2063.

nerient directement ces deux observations combinées ensemble, on doit trouver, pour la quantité qu'on cherche, deux résultats différens, et qui différeront d'autant plus que la supposition qu'on aura faite sera plus éloignée de la véritable. On prend donc alors un milieu entre les deux résultats et l'on s'imagine avoir trouvé la vérité; mais il me semble qu'il est très permis d'en douter, car, outre qu'il y a bien des cas où l'on ne peut pas regarder le résultat moyen comme le véritable, ici ce n'est pas même admissible, puisque le milieu du passage et la plus courte distance des centres sont deux quantités qui dépendent l'une de l'autre, et qu'il est impossible de les supposer telles précisément qu'elles se conviennent relativement à deux observations déterminées, à moins que ce ne soit un effet du hazard. Or si je suppose maintenant qu'on prenne les deux mêmes observations, et qu'on les combine ensemble, il est clair qu'on ne trouvera qu'un seul résultat pour la quantité cherchée, mais ce sera précisément la même qu'on aurait eue par un milieu entre les deux résultats trouvés suivant l'autre manière si l'on avait fait une supposition qui s'écartât peu de celle qu'il convenait de faire. Il s'en suit donc qu'on serait arrivé au même but par les deux methodes, mais avec cette différence, que les quantités déterminées d'après la méthode des combinaisons sont dans tous les cas de vrais résultats tels que les donne véritablement l'observation, tandisque d'après l'autre ici ne sont que des résultats fictifs ou approchés. Le calcul devient de la vérité plus long, lorsqu'il y a plus de trois observations; parceque alors le nombre des combinaisons qu'on en peut faire deux à deux, augmentant aussi le nombre des résultats qui en proviendront, surpassera toujours celui des observations. Or si pour déterminer une quantité quelconque d'après une méthode on risque de trouver des résultats inexacts, et que d'après une autre méthode

thode on peut déterminer la même quantité sans courir ce danger, il est incontestable que celle-ci est préférable à l'autre. Lorsqu'on ne peut avoir que des observations isolées, il faut bien alors se résoudre à les calculer séparément, mais encore avec la restriction que les quantités qu'on supposera connues soient données par d'autres observations, qui étant dans le cas d'être combinées deux à deux, soient elles-mêmes très exactes. Il est donc aisé de conclure de tout ce que je viens de dire que la manière de calculer séparément chaque observation, non seulement ne procure pas les avantages qu'on en attend pour la multiplicité des résultats, mais elle est encore moins exacte que celle de combiner deux à deux les observations, ainsi que l'enseignent les plus célèbres astronomes. Je ne me suis permis d'entrer dans ces détails que pour prouver à la Société Royale que je ne me sers jamais avec confiance d'aucune méthode sans l'avoir examinée auparavant en la créant, pour ainsi dire, une seconde fois.

6. J'ai calculé le lieu du soleil et de Mercure par les tables de Halley pour 2 h 31 h. et 4 h. intervalle de tems qui comprend à peu près par son milieu toute la durée du passage, et j'ai trouvé,

	à 2h 30' tems vrai	à 3 h. 30' t. v.	à 4 h. 30' t. v.
	S. ° ' "	S. h. ' "	S. ° ' "
La longitude du soleil de	7 20 22 43,6	7 20 25 14,8	7 20 27 45,9
Son ascension droite	7 17 55 55,3	7 17 58 28,4	7 18 1 1,5
Sa déclinaison - - australe	17 51 49,6	17 52 29,9	17 53 10,1
La longit. géocentrique de Mercure	7 20 32 2,9	7 20 28 40,8	7 20 25 18,4
Sa latitude - - - boréale	0 14 31,0	0 15 22,6	0 16 13,8

Ce qui me donne	entre 2½ h. et 3½ h	entre 3½ h. et 4½ h
	° ' "	° ' "
Le mouvem horaire relatif Merc. sur l'écliptique de	5 33, 3	5 53, 5
L'inclinaison de l'orbite relative sur l'écliptique de	8 18 33, 8	8 14 28, 5
Le mouvem. horaire relatif de Merc. dans son orbite	5 57,05	5 57,19

Je

Je me suis servi de l'inclinaison et du mouvement horaire qui avait lieu entre 2½ h. et 3½ h. dans le calcul des observations du commencement, et l'inclinaison avec le mouvement horaire qui avait lieu entre 3½ h. et 4½ h. m'a servi pour la fin du passage. Quant aux autres élémens, j'ai employé le diamètre du soleil de 32′ 24″,5 ; celui de Mercure de 9″,535 comme je l'ai mesuré sur le disque du soleil pendant le passage, et la parallaxe horizontale du soleil dans ses moyennes distances de 8″,7 telle que je l'ai établie dans mon Mémoire sur le passage de Venus en 1769. D'où j'ai conclu la différence des parallaxes horizontales du soleil et de Mercure pour le jour du passage 12 Novembre de 4″,088.

7. Avec ces élémens j'ai calculé les observations des contacts en ne négligeant pas même les millièmes de seconde dans certains cas ; je n'ai mis cette scrupuleuse exactitude dans tous mes calculs que parceque je voulais m'assurer dans le cas où je viendrais à trouver des différences entre les résultats de même dénomination que je n'eusse à les attribuer uniquement qu'aux observations. La table suivante renferme les résultats les plus importans de ces calculs.

Table

Table des réſultats du calcul des obſervations des contacts et du centre de Mercure.

	Contacts intérieurs	Contacts extér.	Centre de ☿.
	h. ′ ″	h. ′ ″	h. ′ ″
Heure vraie de l'obſervation entrée	3 2 3, 8	2 56 28, 8	2 58 28,8
Heure vraie de l'obſervation ſortie	4 17 18, 4	4 22 53, 4	4 20 36,4
Durée donnée directement par l'obſ.	1 15 14, 6	1 26 24, 6	1 22 7,6
Plus courte diſtance des centres vue à la ſurface de la terre	15 41, 2	15 42, 5	15 41,0
Heure vraie du milieu du paſſage pour le centre de la terre	3 39 47, 4	3 39 47, 1	3 39 38,7
Plus courte diſtance des centres vue du centre de la terre	15 45, 1	15 46, 4	15 44,9
Reduction de l'obſervation entrée au centre de la terre	+ 2 59,45	+ 2 34,38	+ 2 42,9
Reduction de l'obſervation ſortie au centre de la terre	— 2 46,70	— 2 22,27	— 2 30,7
Heure vr. de l'obſervation entrée arrivée pour le centre ♂	3 5 3,25	2 59 3,11	3 1 11,7
Heure vr. de l'obſervation ſortie arrivée pour le centre ♂	4 14 31,70	4 20 31,13	4 18 5,7
Durée du paſſage pour le centre de la terre	1 9 28,45	1 21 28,02	1 16 54,0
Heure vraie de la conjonction de Mercure et du ſoleil	4 2 53, 2	4 2 54, 8	4 2 44,1
Latitude de ☿ en conjonction donnée par obſervation	S. ° ′ ″ 15 55,1	S. ° ′ ″ 15 56,4	S. ° ′ ″ 15 54,8
Longitude du ſoleil ou de Mercure en conjonction	7 20 26 37,6	7 20 26 37,7	7 20 26 37,2
Longitude de Mercure en conjonction donnée par les tables eu égard à l'aberration	7 20 27 8,4	7 20 27 8,3	7 20 27 8,9
Latitude de ☿ en ♂ donnée par les tables - - - boréale	15 50,7	15 50,7	15 50,5
Erreur des tables en longitude	— 30,8	— 30,6	— 31,7
Erreur des tables en latitude	+ 4,4	+ 5,7	+ 4,4

En adoptant la latitude de ☿ au moment de la ♂ donnée par les contacts intérieurs de 15′ 55″,1 je trouve le
lieu du ☊ { S. ° ′ ″ } ° ′ ″
de ☿ à { 1 15 45 22,8 en ſuppoſant l'inclinaiſon de l'orbite 7 0 0 avec M. CASSINI.
{ 1 15 44 55,7 en ſuppoſant l'inclinaiſon de l'orbite 6 59 20 avec HALLEY.

8. L'on

8. L'on voit par cette table que les contacts intérieurs donnent l'heure du milieu du passage à 3 dixièmes de seconde près la même que les contacts extérieurs; l'heure de la conjonction à 1″,6 près la même*, et la plus courte distance des centres ainsi que la latitude de Mercure en conjonction de 1″,3 plus petite. Quant aux deux observations du centre de Mercure sur les bords du soleil, elles donnent le milieu du passage de 8″,7 plutôt que les contacts intérieurs, et la plus courte distance des centres ainsi que la latitude de Mercure en conjonction de 2 dixièmes de seconde seulement plus petite. Cette différence dans l'heure du milieu du passage ne peut venir que de la manière dont j'ai estimé le centre de Mercure; car il y a d'abord seconde pour seconde le même intervalle de tems entre les deux contacts de l'entrée qu'entre ceux de la sortie, c'est à dire l'un et l'autre de 5′ 35″. Ensuite je trouve qu'il s'était écoulé 2′ 0″ depuis le contact extérieur de l'entrée jusqu'au moment où j'ai estimé le centre de Mercure sur le bord du soleil, au lieu de 2′ 17″ qu'il y a entre les pareilles observations de la sortie; mais cet intervalle de tems devant être le même pour l'entrée et pour la sortie, la différence 17″ fait voir que j'ai estimé le centre de Mercure plus près du contact extérieur à l'entrée qu'à la sortie, ce qui devait aussi avancer l'instant du milieu du passage; or la moitié de ces 17″ fait précisément les 8′ ½ dont le milieu du passage est arrivé plutôt selon cette observation que selon celle des contacts intérieurs (puisque l'erreur de l'une des deux observations n'est que la moitié sur le milieu du passage). J'ai donc marqué l'instant de l'observation du centre à l'entrée plutôt qu'il

* L'instant de la conjonction diffère de 1″,6 quoique celui du milieu du passage ne diffère que de 0″,3, parceque la portion de l'orbite relative comprise entre le milieu du passage et la conjonction est plus grande pour une plus grande distance des centres.

ne

ne fallait ; car je pencherai toujours à croire plutôt que c'eſt ſur celle de l'entrée que doit tomber l'erreur, parceque n'ayant pas encore vu Mercure ſur le diſque du ſoleil, je ne pouvais pas juger de ſa grandeur auſſi bien qu'à la fin après l'avoir vu pendant toute la durée de ſon paſſage. C'eſt auſſi en partie par cette même raiſon, jointe à celle qu'on ne peut pas eſtimer avec quelque préciſion le centre d'un corps qu'on ne voit pas entièrement, que je puis avoir obſervé le centre de Mercure ſur le bord du ſoleil trop tôt à l'entrée, et trop tard à la ſortie relativement aux obſervations des contacts. Cette diſcuſſion, en apparence d'ailleurs peu importante, devient ici d'une grande néceſſité, parcequ'il s'agit de montrer les défauts de deux obſervations que je ne rejette qu'avec beaucoup de regrets ; car l'obſervation du centre de la planète ſur le bord du ſoleil n'étant pas affectée de l'effet de pluſieurs élémens (le diamètre de la planète et l'effet d'une atmoſphère qui l'envelopperait) que nous connaiſſons ſouvent mal, ou que nous ignorons abſolument, offrirait des avantages réels, ſi elle pouvait ſe faire avec une certaine préciſion.

9. Quoique les réſultats de mes calculs s'accordent aſſez pour inſpirer quelque confiance, je n'ai cependant pas été trop ſatisfait de trouver la plus courte diſtance des centres de 1″,3 plus grande par les contacts extérieurs que par les contacts intérieurs. Cette différence annonce une erreur dans les durées. Ou la durée du paſſage entre les deux contacts extérieurs eſt trop petite, ou celle des contacts intérieurs eſt trop grande. Mais je me ſuis impoſé la loi de ne jamais faire aucune correction à mes obſervations lorſque je ne les ai accompagnées d'aucune marque qui me faſſe douter de leur bonté ; je ne trouve donc aucune raiſon qui m'autoriſe à changer la durée des contacts intérieurs, et quand je voudrais m'écarter ici un moment de mes

principes pour augmenter la durée des contacts extérieurs, je ne le pourrais faire qu'en considération de l'incertitude avec laquelle on peut estimer le contact extérieur de l'entrée trop tard, et celui de la sortie trop tôt, ce qui est toujours probable ; mais je ne la pourrais augmenter que tout au plus de 5 à 6 secondes de tems, puisqu'on a vu dans l'article précédent qu'il n'y a que 17″ d'incertitude sur l'estime des deux observations du centre de Mercure sur les bords du soleil qui comparativement entre elles-mêmes se font beaucoup moins exactement. Or ces 5 ou 6 secondes d'augmentation sur la durée extérieure ne suffisent pas à beaucoup près (car il en faudrait 106″) pour réduire à zéro la différence qui se trouve entre les deux valeurs de la plus courte distance des centres. Il faut donc chercher ailleurs que dans les observations la cause de cette différence. C'est ce que je crois pouvoir trouver dans l'effet d'une atmosphère supposée autour de Mercure, ou d'une cause semblable.

10. D'après les recherches que j'ai faites sur l'atmosphère de Venus à l'occasion de son passage en 1769, et dont j'ai établi et démontré les principes dans un petit Traité complet sur les passages de Venus et de Mercure, j'étais prévenu que la circonstance caractéristique de ce passage de Mercure qui était si desavantageuse à l'égard de l'utilité qu'on en retire pour perfectionner les tables, devait être extrêmement favorable à la détermination de l'effet d'une atmosphère qui environnerait Mercure, puisque la planète passant fort près du bord du soleil, son mouvement se faisait très obliquement à ce bord, et agrandissait beaucoup l'effet d'une atmosphère. En conséquence je me suis singulièrement appliqué à observer ce passage et principalement les quatre contacts avec la plus grande attention, afin de me procurer des observations suffisamment exactes pour pouvoir m'en servir avec avantage à déterminer l'effet de cette atmo-

sphère,

Iphère, ou du moins à m'assurer de son existence. Je puis dire maintenant que les résultats de mes calculs, de quelque manière que je les combine, en supposant l'observation et le diamètre de Mercure employé dans mes calculs rigoureusement exacts, m'indiquent la présence d'un effet semblable à celui d'une réfraction ou inflexion que souffriraient les rayons solaires dans leur passage auprès du globe de Mercure. Voici comment.

11. J'ai démontré dans le petit Traité que je viens de citer que la combinaison des deux observations des contacts extérieurs doit donner le même instant pour celui du milieu du passage que la combinaison des deux contacts intérieurs, et que cet instant du milieu du passage déduit de l'une et de l'autre combinaison restera toujours absolument le même, qu'on suppose la planète entourée d'une atmosphère ou non. Il est évident qu'à plus forte raison le milieu du passage déduit de la combinaison des deux observations du centre de la planète sur le bord du soleil ne sera point altéré par l'effet d'une atmosphère, puisqu'elle n'influe pas même sur chacune de ces deux observations séparément. Ensuite j'ai encore fait voir que dans la supposition d'une atmosphère autour de la planète qui passe sur le disque du soleil, le milieu du passage déduit de la combinaison de l'observation du contact extérieur de l'entrée avec celle du contact intérieur de la sortie, doit arriver plus tard; et le milieu du passage donné par la combinaison du contact intérieur de l'entrée avec le contact extérieur de la sortie, doit arriver précisément de la même quantité plutôt que le milieu du passage conclu par la combinaison des deux contacts intérieurs, ou par celle des deux contacts extérieurs, ou, ce qui revient encore au même, que le milieu du passage que donneraient indistinctement toutes les observations des contacts combinées comme on voudra, si la planète n'avait point d'atmosphère. La différence

ou la quantité, dont le milieu du passage est trouvé plus tard ou plutôt, sera l'effet de l'atmosphère de la planète sur le milieu du passage.

12. En conséquence de ces principes j'ai donc fait encore deux combinaisons pour en déduire le milieu du passage, et j'ai trouvé que la combinaison du contact extérieur de l'entrée avec le contact intérieur de la sortie donne cet instant à 3h. 40′ 13″,6; celle du contact interieur de l'entrée avec le contact extérieur de la sortie le donne à 3 h. 39′ 20″,8. Or on a vu (art. 7.) que le milieu du passage, selon la combinaison des deux contacts intérieurs et celle des deux contacts extérieurs est arrivé à 3 h. 39′ 47″,2, quantité qui se trouve entre les deux précédentes et exactement à égales distances de l'une et de l'autre, savoir de 26″,4. Il est donc évident que l'effet de l'atmosphère de Mercure dans ce passage-ci a été 26″,4 de tems sur le milieu du passage, en faisant abstraction de toute autre cause qui peut avoir quelque influence sur les observations des contacts.

13. Mais ces 26″,4 ne peuvent provenir que de trois causes : ou de l'inexactitude des observations, ou d'une erreur sur les diamètres du soleil et de Mercure employés dans les calculs, ou de la réfraction des rayons solaires dans l'atmosphère de Mercure; ainsi que je l'ai démontré dans mon petit Traité sur les Passages de Venus et de Mercure, et où j'arrive, après un examen rigoureux de toutes les hypothèses possibles, à cette équation générale $A = \alpha \pm \beta \pm \gamma \pm \iota$; dans laquelle A est la quantité déterminée par les combinaisons des observations, comme ici les 26″,4, et par conséquent connue; α, β, γ, ι, la part qui en appartient respectivement à l'atmosphère de la planète, à l'erreur de son diamètre, à celle du soleil et à l'erreur de l'observation. Je ferai remarquer seulement au sujet de cette formule qu'il n'y a que l'erreur sur le diamètre de la planète dont l'effet β pourrait quelquefois entrer

dans

dans la valeur de A comme quantité négative, mais alors, loin de nuire à l'opinion d'attribuer cet effet, qui est ici de 26″,4, à l'atmosphère de la planète, elle la favoriserait plutôt. Quant à l'erreur sur le diamètre du soleil, son influence peut être regardée comme nulle dans tous les cas, c'est à dire γ peut toujours être regardé comme zéro, à moins que l'erreur sur le diamètre du soleil ne soit très considérable, et c'est un des avantages de ma méthode pour déterminer la valeur de A. Or la probabilité serait en faveur des observations, puisqu'elles donnent, ce qui est conforme à la théorie, le même intervalle de tems entre les deux contacts de l'entrée qu'entre les deux contacts de la sortie, ainsi l'on aurait ici $\varepsilon = 0$. Quant aux deux autres causes, il n'en est pas de même, puisqu'il est évident par la formule générale qu'une même quantité considérée comme erreur sur les diamètres, ou comme réfraction des rayons solaires dans l'atmosphère de Mercure, est capable de produire exactement le même effet. Mais comme il est très probable que les trois causes à la fois peuvent avoir concouru à produire ces $26'',4 = A$, et qu'il est absolument impossible, d'après ma méthode comme d'après toute autre, de démêler les effets pour assigner à chaque cause la part qui lui appartient dans la valeur de A, le problème restera indéterminé à cet égard, par conséquent si l'on ne veut admettre qu'une seule cause, on sera libre de se décider pour l'une ou pour l'autre; or la question n'étant plus alors qu'une affaire d'opinion, le choix doit tomber nécessairement sur la cause qui est la moins connue, et dont nous ne pouvons pas raisonnablement contester l'existence. On peut donc fort bien attribuer cet effet à l'atmosphère de Mercure sans craindre de se tromper beaucoup. Il s'en suit donc qu'en regardant ces 26″,4 simplement comme effet de l'atmosphere de Mercure, la quantité, qui en résulterait pour l'inflexion ou la

réfraction

réfraction réelle de cette atmosphère, nous assurerait au moins d'une espece de limite qu'elle ne surpasserait jamais ou du moins très-rarement, puisque l'inflexion des rayons solaires, à elle-seule, ne peut égaler la somme des trois causes dont elle fait partie, que dans l'hypothèse particulière des deux autres égales à zéro. Cette manière d'envisager le problème me donnera du moins une connaissance approchée de la valeur de la réfraction de l'atmosphère de Mercure, dont je n'aurais sans cette recherche absolument aucune idée. Or il me semble qu'il vaut mieux acquérir une connaissance imparfaite que de rester dans l'ignorance absolue.

14. La quantité de cet effet, quel qu'il soit, étant donc connue, j'ai cherché à concilier les deux valeurs de la plus courte distance des centres trouvées par les contacts intérieurs et extérieurs, et pour cet effet je me suis proposé ce problème qui doit s'en suivre naturellement, puisque la valeur de y peut toujours être regardée comme zéro : *Déterminer le diamètre du soleil, celui de Mercure étant connu par observation, tel que la durée donnée par les contacts extérieurs et la durée des contacts intérieurs fassent trouver, l'une et l'autre, la même quantité pour valeur de la plus courte distance des centres.* Ce problème étant resolu en nommant a, la demi-durée entre les contacts extérieurs, b celle des contacts intérieurs, δ le diametre de Mercure, x la différence des demi-diamètres de Mercure et du soleil, y la plus courte distance des centres cherchée, je trouve $x = \frac{a^2 - b^2 - \delta^2}{2\delta}$ et $y = \sqrt{\left(\frac{a^2 - b^2 - \delta^2}{2\delta}\right)^2 - b^2}$; formules qui étant évaluées après avoir convenablement corrigé des 26,″4 chaque observation des quatre contacts, et augmenté la durée des contacts extérieurs de ces 6″ dont j'ai parlé ci-devant, me donnent $x = 967'',04$ valeur plus

petite de 0″,44 que celle que j'avais ſuppoſée dans mes calculs, et $\gamma = 15'45''{,}24$ plus grande de 0″,1 que celle qui m'a été donnée par la combinaiſon des deux contacts intérieurs; ainſi tous les réſultats trouvés par cette combinaiſon n'auront beſoin d'aucune correction, et je les adopterai, comme étant les meilleurs, tels qu'ils ſont rapportés dans la table ci-deſſus art. 7.

15. En ſuppoſant donc que les 26″,4 ſoient produites par l'atmoſphère de Mercure, je trouve 0″276 pour la réfraction horizontale de cette atmoſphère. Les obſervations du paſſage de Venus en 1769 m'ayant annoncé un effet ſemblable d'environ 8″ à 9″ de tems, je trouve ſa réfraction horizontale d'environ 0″205 qui n'eſt qu'à peu près les deux tiers de celle de Mercure.

CONCLUSION.

16. Quelque peu de confiance que j'attache à ces réſultats, et quelque ſoit l'opinion que j'adopte pour choiſir entre les cauſes qui peuvent produire l'effet en queſtion, je crois du moins pouvoir conclure avec certitude, ce que je m'étais principalement propoſé de prouver dans ce Mémoire, que les obſervations dont il s'agit ici, malgré le degré d'incertitude qu'on puiſſe leur ſuppoſer, indiquent clairement l'exiſtence d'un effet ſemblable à celui d'une atmoſphère qui environnerait la planète; et que cet effet, ſoit qu'il provienne effectivement de cette atmoſphère, ou d'une erreur ſur le diamètre de la planète, ou d'une erreur dans les obſervations, ou qu'il ſoit le réſultat de l'action ſimultanée des trois cauſes réunies, il ſe fait ſentir évidemment dans toutes les obſervations des paſſages de Venus et de Mercure, du moins dans toutes celles que j'ai calculées. Par conſéquent l'influence de ces cauſes qui altèrent les obſervations d'une maniere ſi ſenſible me parait, ſous tous les points de vue, mériter l'attention des aſtronomes; et je ſuis très perſuadé que faute d'y avoir eu

égard

égard dans la comparaison des observations du passage de Venus pour en déduire la parallaxe du soleil, bien des astronomes seraient dans le cas de recommencer leur calcul. Heureusement je n'ai pas ce reproche à me faire; car j'ai constamment évité avec le plus grand soin l'effet d'une atmosphère autour de Venus en choisissant les observations pour en faire la comparaison de manière que l'effet de cette atmosphère, qu'il ait existé ou non, se trouvait toujours réduit à zéro. C'est ainsi que dans mon Mémoire sur le passage de Venus en 1769 j'ai fixé à 8″,7 la parallaxe horizontale du soleil dans ses moyennes distances à la terre.

XXV. *Thoughts on the conſtituent Parts of Water and of Dephlogiſticated Air; with an Account of ſome Experiments on that Subject. In a Letter from Mr.* James Watt, *Engineer, to Mr.* De Luc, *F. R. S.*

Read April 29, 1784.

DEAR SIR, Birmingham, November 26, 1784.

IN compliance with your deſire, I ſend you an account of the hypotheſis I have ventured to form on the probable cauſes of the production of water from the deflagration of a mixture of dephlogiſticated and inflammable airs, in ſome of our friend Dr. PRIESTLEY's experiments.

I feel much reluctance to lay my thoughts on theſe ſubjects before the public in their preſent indigeſted ſtate, and without having been able to bring them to the teſt of ſuch experiments as would confirm or refute them; and ſhould, therefore, have delayed the publication of them until theſe experiments had been made, if you, Sir, and ſome other of my philoſophical friends, had not thought them as plauſible as any other conjectures which have been formed on the ſubject; and that though they ſhould not be verified by further experiments, or approved of by men of ſcience in general, they may perhaps merit a diſcuſſion, and give riſe to experiments which may throw light on ſo important a ſubject.

I firſt thought of this way of ſolving the phænomena in endeavouring to account for an experiment of Dr. PRIEST-

LEY's, wherein water appeared to be converted into air; and I communicated my ſentiments in a letter addreſſed to him, dated April 26, 1783*, with a requeſt that he would do me the honour to lay them before the Royal Society; but as, before he had an opportunity of doing me that favour, he found, in the proſecution of his experiments, that the apparent converſion of water into air, by expoſing it to heat in porous earthen veſſels, was not a real tranſmutation, but an exchange of the elaſtic fluid for the liquid, in ſome manner not yet accounted for; therefore, as my theory was no ways applicable to the explaining theſe experiments, I thought proper to delay its publication, that I might examine the ſubject more deliberately, which my other avocations have prevented me from doing to this time.

1. It has been known for ſome time, that inflammable air contained much phlogiſton; and Dr. PRIESTLEY has found, by ſome experiments made lately, that it "is either wholly "pure phlogiſton, or at leaſt that it contains no apparent mix-"ture of any other matter." (In my opinion, however, it contains a ſmall quantity of water and much elementary

* This letter Dr. PRIESTLEY received at London; and, after ſhewing it to ſeveral Members of the Royal Society, he delivered it to Sir JOSEPH BANKS, the Preſident, with a requeſt that it might be read at ſome of the public meetings of the Society; but before that could be complied with, the author, having heard of Dr. PRIESTLEY's new experiments, begged that the reading might be delayed. The letter, therefore, was reſerved until the 22d of April laſt; when, at the author's requeſt, it was read before the Society. It has been judged unneceſſary to print that letter, as the eſſential parts of it are repeated, almoſt *verbatim*, in this letter to M. DE LUC; but, to authenticate the date of the author's ideas, the parts of it which are contained in the preſent letter are marked with double commas.

heat.)

heat *.) "He found, that by expoſing the calces of metals "to the ſolar rays, concentrated by a lens, in a veſſel contain- "ing inflammable air only, the calces of the ſofter metals "were reduced to their metallic ſtate;" and that the inflammable air was abſorbed in proportion as they became phlogiſticated; and, by continually ſupplying the veſſel with inflammable air, as it was abſorbed, he found, that out of 101 ounce meaſures, which he had put into the veſſel, 99 ounce meaſures were abſorbed by the calces, and only two ounce meaſures remained, which, upon examination, he found to be nearly of the ſame quality the whole quantity had been of before the experiment, and to be ſtill capable of deflagrating in conjunction with atmoſpheric or with dephlogiſticated air. *Therefore, as ſo great a quantity of inflammable air had been abſorbed by the metallic calces; the effect of reducing them to their metallic ſtate had been produced; and the ſmall remaining portion was ſtill unchanged, at leaſt had ſuffered no change which might not be attributed to its original want of purity; it was reaſonable to conclude, that inflammable air muſt be the pure phlogiſton, or the matter which reduced the calces to metals.*

2. "The ſame ingenious philoſopher mixed together cer- "tain proportions of pure dry dephlogiſticated air and of pure "dry inflammable air in a ſtrong glaſs veſſel, cloſely ſhut, "and then ſet them on fire by means of the electric ſpark," in the ſame manner as is done in the inflammable air piſtol. "The firſt effect was the appearance of red heat or inflamma-

* Previous to Dr. PRIESTLEY's making theſe experiments, M. KIRWAN had proved, by very ingenious deductions from other facts, that inflammable air was, in all probability, the real phlogiſton, in an aerial form. Theſe arguments were perfectly convincing to me; but it ſeems more proper to reſt that part of the preſent hypotheſis on the direct experiment.

" tion in the airs, which was ſoon followed by the glaſs veſſel " becoming hot. The heat gradually pervaded the glaſs, and " was diſſipated in the circumambient air, and as the glaſs " grew cool, a miſt or viſible vapour appeared in it, which " was condenſed on the glaſs in the form of moiſture or dew *. " When the glaſs was cooled to the temperature of the atmo- " ſphere, if the veſſel was opened with its mouth immerſed in " water or mercury, ſo much of theſe liquids entered, as was " ſufficient to fill the glaſs within about $\frac{1}{100}$dth part of its " whole contents; and this ſmall reſiduum may ſafely be con- " cluded to have been occaſioned by ſome impurity in one or " both kinds of air. The moiſture adhering to the glaſs, after " theſe deflagrations, being wiped off, or ſucked up, by a " ſmall piece of ſponge paper, firſt carefully weighed, was " found to be exactly, or very nearly, equal in weight to the " airs employed."

" In ſome experiments, but not in all, a ſmall quantity of " a ſooty-like matter was found adhering to the inſide of the " glaſs," the origin of which is not yet inveſtigated; but Dr. PRIESTLEY thinks, that it ariſes from ſome minute grains of the mercury that was uſed in order to fill the glaſs with the air, which being ſuper-phlogiſticated by the inflammable air, aſſumed that appearance; but, from whatever cauſe it proceeded, " the whole quantity of ſooty-like matter was too " ſmall to be an object of conſideration, particularly as it did " not occur in all the experiments."

I am obliged to your friendſhip for the account of the experiments which have been lately made at Paris on this ſubject,

* I believe that Mr. CAVENDISH was the firſt who diſcovered that the combuſtion of dephlogiſticated and inflammable air produced moiſture on the ſides of the glaſs in which they were fired.

with

with large quantities of theſe two kinds of air, by which the eſſential point ſeems to be clearly proved, that the deflagration or union of dephlogiſticated and inflammable air, by means of ignition, produces a quantity of water equal in weight to the airs; and that the water, thus produced, appeared, by every teſt, to be pure water. As I am not furniſhed with any particulars of the manner of making the experiment, I can make no obſervations on it, only that, from the character you give me of the gentlemen who made it, there is no reaſon to doubt of its being made with all neceſſary precautions and accuracy, which was farther ſecured by the large quantities of the two airs conſumed.

3. "Let us now conſider what obviouſly happens in the "caſe of the deflagration of the inflammable and dephlogiſti-"cated air. Theſe two kinds of air unite with violence, they "become red-hot, and upon cooling totally diſappear. When "the veſſel is cooled, a quantity of water is found in it equal "to the weight of the air employed. This water is then the "only remaining product of the proceſs, and *water*, *light*, and "*heat*, are all the products," unleſs there be ſome other matter ſet free which eſcapes our ſenſes.

"*Are we not then authoriſed to conclude, that water is com-*"*poſed of dephlogiſticated air and phlogiſton, deprived of part of*"*their latent or elementary heat; that dephlogiſticated or pure air*"*is compoſed of water deprived of its phlogiſton, and united to*"*elementary heat and light; and that the latter are contained in*"*it in a latent ſtate, ſo as not to be ſenſible to the thermometer or*"*to the eye; and if light be only a modification of heat, or a cir-*"*cumſtance attending it, or a component part of the inflammable*"*air, then pure or dephlogiſticated air is compoſed of water de-*"*prived of its phlogiſton and united to elementary heat?*"

4. "It

4. "It appears, that dephlogiſticated water," or, which may be a better name for the baſis of water and air, the element you call *humor*, "has a more powerful attraction for "phlogiſton than it has for latent heat, but that it cannot "unite with it, at leaſt not to the point of ſaturation, or to "the total expulſion of the heat, unleſs it be firſt made red-"hot," or nearly ſo. "The electric ſpark heats a portion o "it red-hot, the attraction between the humor and the phlo-"giſton takes place, and the heat which is let looſe from this "firſt portion heats a ſecond, which operates in a like manner "on the adjoining particles, and ſo continually until the whole "is heated red-hot and decompoſed." Why this attraction does not take place to the ſame degree in the common temperature of the atmoſphere, is a queſtion I am not yet able to ſolve; but it appears, that, in ſome circumſtances, "dephlo-"giſticated air can unite, in certain degrees, with phlogiſton "without being changed into water." Thus Dr. PRIESTLEY has found, that by taking clean filings of iron, which, alone, produce only inflammable air of the pureſt kind, and *mercurius calcinatus per ſe*, which gives only the pureſt dephlogiſticated air, and expoſing them to heat, in the ſame veſſel, he obtained neither dephlogiſticated nor inflammable air, "but in their "place fixed air." Yet it is well known, that a mixture of dephlogiſticated and inflammable air will remain for years in cloſe veſſels in the common heat of the atmoſphere, without ſuffering any change, the mixture being as capable of deflagration at the end of that time as it was when firſt ſhut up. Theſe facts the Doctor accounts for, by ſuppoſing that the two kinds of air, when formed at the ſame time in the ſame veſſel, can unite in their *naſcent* ſtate; but that, when fully formed, they are incapable of acting upon one another, unleſs they are

firſt

firſt ſet in motion by external heat. "Phlogiſticated air ſeems "alſo to be another compoſition of phlogiſton and dephlogiſti- "cated air;" but in what proportions they are united, or by what means, is ſtill unknown. It appears to me to be very probable, that fixed air contains a greater quantity of phlogiſton than phlogiſticated air does, becauſe it has a greater ſpecific gravity, and becauſe it has more affinity with water.

5. "For many years I have entertained an opinion, that air "was a modification of water, which was originally founded "on the facts that in moſt caſes, wherein air was actually "made," which ſhould be diſtinguiſhed from thoſe wherein it is only extricated from ſubſtances containing it in their pores, or otherwiſe united to them in the ſtate of air, "the ſub- "ſtances were ſuch as were known to contain water as one of "their conſtituent parts, yet no water was obtained in the "proceſſes," except what was known to be only looſely connected with them, ſuch as the water of the cryſtallization of ſalts. "This opinion aroſe from a diſcovery," that the latent heat contained in ſteam diminiſhed in proportion as the ſenſible heat of the water from which it was produced increaſed; or, in other words, "that the latent heat of ſteam was leſs "when it was produced under a greater preſſure, or in a more "denſe ſtate, and greater when it was produced under a leſs "preſſure, or in a leſs denſe ſtate; which led me to conclude, "that when a very great degree of heat was neceſſary for the "production of the ſteam, the latent heat would be wholly "changed into ſenſible heat; and that, in ſuch caſes, the "ſteam itſelf might ſuffer ſome remarkable change. I now "abandon this opinion in ſo far as relates to the change of "water into air, as I think that may be accounted for on better "principles."

6. "In every caſe, wherein dephlogiſticated air has been "produced, ſubſtances have been employed, ſome of whoſe "conſtituent parts have a ſtrong attraction for phlogiſton, and, "as it would appear, a ſtronger attraction for that ſubſtance "than *humor* has; they ſhould, therefore, dephlogiſticate the "water" or fixed air, and the *humor* thus ſet free ſhould unite to the matter of fire and light and become pure air. Dephlogiſticated air is produced in great abundance from melted nitre. "The acid of nitre has a greater attraction for phlogiſton than "any other ſubſtance is known to have; and it is alſo certain, "that nitre, beſides its water of cryſtallization, contains a "quantity of water as one of its elementary parts, which "water adheres to the other parts of the nitre with a force "ſufficient to enable it to ſuſtain a red heat. When the nitre "is melted, or made red-hot, the acid acts upon the water and "dephlogiſticates it; and the fire ſupplies the *humor* with the "due quantity of heat to conſtitute it air, under which form "it immediately iſſues. It is not eaſy to tell what becomes of "the acid of nitre and phlogiſton, which are ſuppoſed to be "united," as they ſeem to be loſt in the proceſs. Dr. PRIESTLEY has lately made ſome experiments, with a view to aſcertain this point. He diſtilled dephlogiſticated air from pure nitre, in an earthen retort glazed within and without. He employed 2 oz. = 960 grains of nitre: the retort was placed in an air furnace, and, by means of an intenſe heat, he obtained from the nitre in one experiment 787, and in another experiment 800 ounce meaſures of dephlogiſticated air; and he found that, upon weighing the retort and nitre before and after the proceſs, they had ſuffered a loſs of weight equal to the weight of the air, and to the water of cryſtallization of the nitre, but nothing more. He remarked, that the air had a pungent ſmell,

smell, which he could not divest it of by washing; and that the water in which the air was received had become slightly acid. I examined a portion of this water, which he was so kind as to send me, and found by it that the whole of the receiving water had contained the acid belonging to 2 drams = 120 grains of nitre. I also examined the residuum and the retort in which the distillation had been performed, and found the residuum highly alkaline, yet containing a minute quantity of phlogisticated nitrous acid. It had acted considerably upon the retort, and had dissolved a part of it, which was deposited in the form of a brownish powder, when the saline part was dissolved in water. This earthy powder I have not yet thoroughly examined, but have no doubt that it principally consists of the earth of the retort. This experiment, and all others tried in earthen vessels, leave us still at a loss to determine what becomes of the acid and phlogiston. They seem either to remain mixed with the air, in the form of an incoercible gas; or to unite with the alkali, or with the earth of the retort, in some manner so as not to be easily separated from them; or else they are imbibed by the retorts themselves, which are sufficiently porous to admit of such a supposition.

All that appears to be conclusive from this experiment is, that above one half of the weight of the nitre was obtained in the form of dephlogisticated air; and that the residuum still contained some nitrous acid united to phlogiston.

7. Finding that the action of the nitre on the retort tended to prevent any accurate examination of the products, I had recourse to combinations of the nitrous acid with earths from which the dephlogisticated air is obtained with less heat than from nitre itself. As these processes have been particularly described by Dr. PRIESTLEY, by Mr. SCHEELE, and others, I

ſhall not enter into any detail of them; but ſhall mention the general phænomena which I obſerved, and which relate to the preſent ſubject.

The earths I uſed were magneſia alba, calcareous earth, and minium or the red calx of lead. I diſſolved them in the reſpective experiments in nitrous acid dephlogiſticated by boiling, and diluted with proper proportions of water. I made uſe of glaſs retorts, coated with clay; and I received the air in glaſs veſſels, whoſe mouths were immerſed in a glazed earthen baſon, containing the ſmalleſt quantity of water that could be uſed for the purpoſe. As ſoon as the retort was heated a little above the heat of boiling water, the ſolutions began to diſtil watery vapours containing nitrous acid. Soon after theſe vapours ceaſed, yellow fumes, and in ſome of the caſes dark red fumes, began to appear in the neck of the retort; and at the ſame time there was a production of dephlogiſticated air, which was greater in quantity from ſome of theſe mixtures than from others, but continued in all of them until the ſubſtances were reduced to dryneſs. I found, in the receiving water &c. very nearly the whole of the nitrous acid uſed for their ſolution, but highly phlogiſticated, ſo as to emit nitrous air by the application of heat; and there is reaſon to believe, that with more precaution the whole might have been obtained.

8. As the quantity of dephlogiſticated air produced by theſe proceſſes did not form a ſufficient part of the whole weight, to enable me to judge whether any of the real acid entered into the compoſition of the air obtained, I ceaſed to purſue them further, having learned from them the fact, *that however much the acid and the earths were dephlogiſticated before the ſolution, the acid always became highly phlogiſticated in the proceſs.*

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In order to examine whether this phlogiſton was furniſhed by the earths, ſome dephlogiſticated nitrous acid was diſtilled from minium till no more acid or air came over. More of the ſame acid was added to the minium as ſoon as it was cold, and the diſtillation repeated, which produced the ſame appearance of red fumes and dephlogiſticated air. This operation was repeated a third time on the ſame minium, without any ſenſible variation in the phænomena. The proceſs ſhould have been ſtill farther repeated, but the retort broke about the end of the third diſtillation. The quantity of minium uſed was 120 grains, and the quantity of nitrous acid added each time was 240 grains, of ſuch ſtrength that it could diſſolve half its weight of mercury, by means of heat.

It appears from this experiment, that unleſs minium be ſuppoſed to conſiſt principally of phlogiſton, the ſource of the phlogiſton, thus obtained, was either the nitrous acid itſelf, or the water with which it was diluted; or elſe that it came through the retort with the light, for the retort was in this caſe red-hot before any air was produced; yet this latter concluſion does not appear very ſatisfactory, when it is conſidered, that in the proceſs wherein the earth made uſe of was magneſia, the retort was not red-hot, or very obſcurely ſo, in any part of the proceſs; and by no means luminous, when the yellow and red fumes firſt made their appearance.

9. As the principal point in view was to determine whether any part of the acid entered into the compoſition of the air, I reſolved to employ ſome ſubſtance which would part with the acid in a moderate heat, and alſo give larger quantities of air than had been obtained in the former proceſſes. Mercury was thought a proper ſubſtance for this purpoſe. 240 grains of mercury were put into a glaſs retort with 480 grains

of diluted dephlogiſticated nitrous acid, which was the quantity neceſſary to diſſolve the whole of the mercury, a gentle heat was applied, and as ſoon as the common air contained in the retort was diſſipated, a veſſel was placed to receive the nitrous air proceeding from the ſolution, which was 16 ounce meaſures. When it had ceaſed to give nitrous air, the neck of the retort became hot from the watery ſteams of the acid. The air receiver was taken away, and a common receiver was luted on, with a little water in it, to condenſe the vapours, and a quantity of dilute, but highly phlogiſticated, acid was caught in the receiver. When the watery vapours had nearly come over, and yellow fumes appeared in the neck of the retort, the common receiver was removed, and the air receiver replaced; about four ounces of very ſtrong nitrous air paſſed up immediately, the fumes in the retort became red, and dephlogiſticated air paſſed up, which, uniting with the nitrous air in the receiver, produced red fumes in the receiver; and the two kinds of air acting upon one another, their bulk was reduced to half of an ounce meaſure. At this period the fumes in the retort were of a dark red colour, and dephlogiſticated air was produced very faſt. After a ſhort time, ſome orange-coloured ſublimate appeared in the upper part of the retort, and extended a little way along its neck, the red colour of the fumes gradually diſappeared, and the neck of the retort became quite clear. At the ſame time that this happened, ſmall globules of mercury appeared in the neck of the retort, and accumulated there until they ran down in drops. The production of the air was now very rapid, and accompanied with much of the white cloud or powdery matter, which paſſed up with the air into the receiver, and mixed with the water, but did not diſſolve in it. After giving about 36 ounce meaſures of dephlogiſticated air,

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it ſuddenly ceaſed to give any more; and the retort being cooled, the bulb was found to be quite empty, excepting a ſmall quantity of black powder, which, on being rubbed on the hand, proved to be moſtly running mercury. The orange-coloured ſublimate was waſhed out of the neck of the retort, and what running mercury was in it was ſeparated, and added to that which had run down into the baſon among the water. The whole fluid mercury, when dried, weighed 218 grains; therefore 22 grains remained in the form of ſublimate, which, I believe, would alſo have been reduced if I could have applied heat in a proper manner to the neck of the retort, as ſome of it, to which heat could be applied, diſappeared.

10. The 16 ounce meaſures of nitrous air, which had been produced in the ſolution of the mercury, and had remained confined by water in the receiver, was converted into nitrous acid by the gradual admiſſion of common air, and was taken up by the water; this water was added to that in the baſon, which had ſerved to receive the dephlogiſticated air. The whole quantity was about two quarts, was very acid to the taſte, and ſparkling with nitrous air. It was immediately put into bottles, and well corked, until it had loſt the heat gained in the operation. In order to determine the quantity of acid in the receiving water and in the ſublimate, I diſſolved, firſt, alkali of tartar in water, and filtered the ſolution. 352 grains of this alkaline ſolution ſaturated 120 grains of the nitrous acid I had employed to diſſolve the mercury, and 1395 grains of the ſame alkaline ſolution ſaturated the orange-coloured precipitate, and all the acid liquor obtained from the proceſs: therefore we have the proportion as 352 : 120 :: 1395 : 475, from which it appears, that all the acid employed was recovered again in the form of acid, excepting only five grains; a ſmaller

a ſmaller quantity than what might reaſonably be ſuppoſed to be loſt in the proceſs by the extreme volatility of the nitrous air. In order to aſcertain the exact point of ſaturation, ſlips of paper, ſtained by the juice of the petals of the ſcarlet roſe, were employed, which were the niceſt teſt I could procure, as litmus will not ſhew the point of ſaturation of any liquor containing much phlogiſticated nitrous acid, or even fixed air, but will turn red, and ſhew it to be acid, when the teſt of thoſe leaves, violets, and ſome other of the like kind, will turn green in the ſame liquor, and ſhew it to be alkaline. But the exact point of ſaturation of ſo dilute a liquor is ſo very difficult to aſcertain, than an error might eaſily be committed, notwithſtanding the attention beſtowed upon it. Suppoſing this experiment to be unexceptionable, the concluſions which may be drawn from it are very favourable to the hypotheſis I endeavour to ſupport. *Thirty-ſix ounce meaſures of dephlogiſticated air were obtained, and only five grains of a weak nitrous acid were loſt in the proceſs. Two hundred and eighteen grains of mercury out of two hundred and forty were revived, and all the dephlogiſticated nitrous acid employed is found to be highly phlogiſticated in the proceſs. It appears, that the nitrous acid does not enter into the compoſition of dephlogiſticated air; it ſeems only to ſerve to abſorb phlogiſton from the watery part of the mercurial nitre.*

11. As this laſt proceſs proved very tedious and complicated on account of the neceſſity of aſcertaining the quantity of acid in the receiving water, by means of an alkali which afforded a double ſource of error in the point of ſaturation, I reſolved to try the diſtillation of dephlogiſticated air from cubic nitre in a glaſs veſſel, and to draw from it only ſuch a quantity of air as it would yield without acting much upon the retort, which

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latter circumſtance is eſſentially neceſſary to be attended to. An ounce of the cryſtals of mineral alkali were diſſolved in nitrous acid, and the mixture brought to an exact ſaturation by the teſt of litmus; 30 ounce meaſures of air were diſtilled from it, which, during the latter part of the proceſs, was accompanied with ſlightly yellow fumes; the receiving water was found to be acid, and the reſiduum alkaline. The reſiduum being diſſolved in the receiving water, the ſolution was neutral, or very nearly ſo, by every teſt; for in this caſe litmus might be uſed, as the acid was very ſlightly phlogiſticated. On adding a few drops of a very dilute nitrous acid, the teſts ſhewed the liquor to be acid.

12. Encouraged by the ſucceſs of this experiment, I took an ounce = 480 grains of pure common nitre, and put it into a flint-glaſs retort, coated, which was placed in a furnace. It began to give air about the time it became red-hot, and during the latter part of the proceſs this air was accompanied with yellowiſh fumes. I ſtopped the proceſs when it had produced 50 ounce meaſures of air. The receiving water, and particularly the air, had a ſtrong but peculiar ſmell of nitrous acid. The air was well waſhed with the receiving water, but was not freed from the ſmell. The receiving water, which was 50 ounces, was ſlightly acid, and the reſiduum alkaline. I diſſolved the latter in the former, and found the mixture alkaline. 10 grains of weak nitrous acid were added to it, which ſaturated it, and 105 grains of this ſpirit of nitre was found to contain the acid of 60 grains of nitre; therefore the 10 grains contained the acid of 5,7 grains of nitre, which, by Mr. KIRWAN's experiments is equal to two grains of real nitrous acid. *We have, therefore, 34 grains weight of dephlogiſticated air produced, and only two grains of real acid miſſing*; and it is not certain

certain that this quantity was deſtroyed, becauſe ſome portion of the glaſs of the retort was diſſolved by the nitre, and ſome part of the materials employed in making the glaſs being alkali, we may conclude, that the alkali of the nitre would be augmented by the alkali of that part of the glaſs it had diſſolved. As the glaſs cracked into ſmall pieces on cooling, and ſome part of the coating adhered firmly to it, the quantity of the glaſs that was diſſolved could not be aſcertained. *From this experiment it appears, that if any of the acid of the nitre enters into the compoſition of the dephlogiſticated air, it is a very ſmall part; and it rather ſeems, that the acid, or part of it, unites itſelf ſo firmly to the phlogiſton as to loſe its attraction for water.*

13. "The vitriolic ſalts alſo yield dephlogiſticated air by "heat; and in theſe caſes the dephlogiſticated air is always "attended with a large quantity of vitriolic acid air or ſul-"phureous vapour," even when the ſalts uſed are not known to contain any phlogiſtic matter. Mr. SCHEELE mentions his having obtained dephlogiſticated air from manganeſe diſſolved in acid of phoſphorus, and alſo from the arſenical acid: from whence it appears, that theſe acids, or perhaps any acid which can bear a red heat, can concur to the production of dephlogiſticated air. *It is neceſſary to remark, that no experiments have been yet publiſhed ſhewing that dephlogiſticated air can be produced from ſalts formed by the muriatic acid. The acids which produce ſalts ſuitable for this purpoſe, have all a ſtrong affinity with phlogiſton; and the marine acid has either a very ſmall affinity with it, or elſe is already ſaturated with it, at leaſt ſo far ſaturated as not to be able to attract it from the* humor.

14. "The dephlogiſticated air obtained from the pure calces "of metals may be attributed to the calces themſelves, attract-"ing the phlogiſton from water which they have imbibed from

" the atmoſphere, or from dephlogiſticating the fixed air which " they are known to contain."

It is very probable, that the dephlogiſticated air extruded from growing vegetables may be owing to their dephlogiſticating the water they grow in; but it appears more probable, that the plants have a power of dephlogiſticating the fixed, or phlogiſticated, air of the atmoſphere.

" When dephlogiſticated and nitrous air are mixed, the de- " phlogiſticated air ſeizes part of the phlogiſton of the nitrous " air." The water contained in the nitrous air, and the other part of the phlogiſton, unite with the nitrous acid, which then aſſumes a liquid form, or at leaſt that of a denſe vapour; " and " that part of the latent heat of the two airs not eſſential to the " new combination is ſet at liberty *."

In the combuſtion of ſulphur the ſame thing happens, but in a greater degree; for the vitriolic acid, having a much weaker attraction for phlogiſton than air has, abandons it almoſt entirely to the latter, which is thereby converted into water, and in that form attracts the vitriolic acid, and reduces it to a liquid ſtate. The ſame reaſoning may be applied to the combuſtion of phoſphorus, which is attended with ſimilar effects.

* I cannot take upon me to determine, from any facts which have come to my knowledge, whether any part of the dephlogiſticated air employed in this experiment is turned into fixed air; but I am rather inclined to think that ſome part is, becauſe the quantity of heat, which is ſeparated by the union of the two airs, does not ſeem to be ſo great as that which is ſeparated when the dephlogiſticated air is wholly changed into water: yet ſome water appears to be formed, becauſe when the mixture is made over mercury, the ſolution of the mercury in the nitrous acid aſſumes a cryſtallized form, which, however, may be due to the watery part of the nitrous air.

15. I ſhall not make, at preſent, any further deductions from what I myſelf conſider ſtill in the light of a conjectural hypotheſis, which I have perhaps dwelt upon too long already. I ſhall only beg your attention to ſome general reaſoning on the ſubject; which, however, may poſſibly ſerve more to ſhew the uncertainty of other ſyſtems on the conſtituent parts of air, than the certainty of this. Some of thoſe ſyſtems ſuppoſe dephlogiſticated air to be compoſed of an acid and ſomething elſe, ſome ſay phlogiſton. If an acid enters into the compoſition of it, why does not that acid appear again when the air is decompoſed, by means of inflammable air and heat? And why is the water which is the product of this proceſs pure water? And if an acid forms one of its conſtituent parts, why has nobody been able to detect any difference in the dephlogiſticated air, made by the help of different acids, when compared with one another, or with the air extruded by vegetables? Theſe airs, of ſuch different origins, appear to be exactly the ſame. And if phlogiſton be a conſtituent part of air, why does it attract phlogiſton with ſuch avidity? Some have, on the other hand, contended that air is compoſed of earth, united to acids or phlogiſton, or to both, or to ſome other matter. Here we muſt aſk, what earth it is which is one of the component parts of air? All earths which will unite with the nitrous or vitriolic acids, and with ſome others, ſuch as the phoſphoric and the arſenical acids, will ſerve as baſes for the formation of air, and the air produced from all of them appears by every teſt to be the ſame, when freed from accidental impurities. To this argument it is anſwered, that it is not any particular ſpecies of earth which is the baſis of air, but elementary or ſimple earth, which is contained in all of them. If this were the

matter

matter of fact, would not that earth be found after the decompoſition of the air?

Mr. SCHEELE has formed an hypotheſis on this ſubject, in which he ſuppoſes heat to be compoſed of dephlogiſticated air united to phlogiſton, and that this combination is ſufficiently ſubtile to paſs through glaſs veſſels. He affirms, that the nitrous and other acids, when in an ignited ſtate, attract the phlogiſton from the heat, and ſet the dephlogiſticated air at liberty; but he does not ſeem to have been more ſucceſsful than myſelf in explaining what becomes of the acid of nitre and phlogiſton in the caſe of the decompoſition of nitre by heat. And ſince we know, from the late experiments, that water is a compoſition of air, or more properly, *humor* and phlogiſton, his whole theory muſt fall to the ground, unleſs that fact be otherwiſe accounted for, which it does not ſeem eaſy to do.

16. To return to the experiment of the deflagration of dephlogiſticated and inflammable air, "it appears from the "two airs becoming red-hot on their union, that the quantity "of heat contained in one or both of them, is much greater "than that contained in ſteam; becauſe, for the firſt moments "after the exploſion, the water depoſited by the air remains in "the form of ſteam, and conſequently retains the latent heat "due to that modification of water. This matter may be eaſily "examined by firing the mixture of dephlogiſticated and inflam-"mable air in a veſſel immerſed in another veſſel containing a "given quantity of water of a known heat, and after the veſſel "in which the deflagration is performed is come to the ſame "temperature with the water in which it is immerſed, by ex-"amining how much heat that water has gained, which being "divided by the quantity of water produced by the decom-"poſition of the airs, will give the whole quantity of elemen-

"tary or latent heat which that water had contained, both as "air and as ſteam; and if from that quantity we deduct the "latent heat of the ſteam, the remainder will be the latent or "elementary heat contained more in air than in ſteam." This experiment may be made more compleatly by means of the excellent apparatus which Meſſ. LAVOISIER and DE LA PLACE have contrived for ſimilar purpoſes.

Until direct experiments are made, we may conclude, from thoſe which have been made by the gentlemen juſt named, on the decompoſitions of air by burning phoſphorus and charcoal, that the heat extricated during the combuſtion of inflammable and dephlogiſticated air is much greater than it appears to be; for they found that one Paris ounce (= 576 Pariſian grains) of dephlogiſticated air, when decompoſed by burning phoſphorus, melted 68,634 ounces of ice; and as, according to another of their experiments, ice, upon being melted, abſorbs 135° of heat, by FAHRENHEIT's ſcale, each ounce of air gave out $68,634 \times 135° = 9265°,590$; that is to ſay, a quantity of heat which would have heated an ounce of water, or any other matter which has the ſame capacity for receiving heat as water has, from 32° to 9265½°: a ſurpriſing quantity! (It is to be underſtood, that all the latent heats mentioned herein are compared with the capacity of water). And when an ounce of dephlogiſticated air was changed into fixed air, by burning charcoal, or by the breathing of animals, it melted 29,547 oz. of ice; conſequently we have $29,547 \times 135° = 3988°,845$. the quantity of heat which an ounce of dephlogiſticated air loſes when it is changed into fixed air. By the heat extricated during the detonation of one ounce of nitre with one ounce of ſulphur, 32 ounces of ice were melted; and, by the experiment I have mentioned of Dr. PRIESTLEY's (6), it appears that

nitre can produce one half of its weight of dephlogisticated air. When the nitre and sulphur are kindled, the dephlogisticated air of the nitre unites with the phlogiston of the sulphur, and sets its acid free, which immediately unites to the alkali of the nitre, and produces vitriolated tartar. The dephlogisticated air, united to the phlogiston, is turned into water, part of which is absorbed by the vitriolated tartar, and part is dissipated in the form of vapours, or unites to the nitrous air, or other air, produced in the process.

As half an ounce of dephlogisticated air is, in this process, united by inflammation to a quantity of phlogiston sufficient to saturate it, and no fixed air is produced, it should melt a quantity of ice equal to the half of what was melted by the combination of an ounce of air with phlogiston in burning phosphorus; that is, it should melt 34,317 ounces of ice; and we find, by Messr. Lavoisier and De la Place's experiment, that it actually melted 32 ounces of ice: the small difference may be accounted for by supposing, that the heat produced by the combustion might not be quite so great as that Dr. Priestley employed in his experiment; or that the nitre might be less pure, and consequently not so much air formed. The two facts, however, agree near enough to permit us to conclude, *that dephlogisticated air, in uniting to the phlogiston of sulphur, produces as much heat as it does in uniting with the phlogiston of phosphorus.*

17. According to Dr. Priestley's experiments, dephlogisticated air unites compleatly with about twice its bulk of the inflammable air from metals. The inflammable air being supposed to be wholly phlogiston, and being $\frac{1}{9,6}$ of the weight of an equal bulk of dephlogisticated air, and being double in quantity, will be $\frac{1}{4,8}$ of the weight of the dephlogisticated air

it

it unites with. Therefore one ounce (576 grains) of dephlogiſticated air, will require 120 grains of inflammable air, or phlogiſton, to convert it into water. And ſuppoſing the heat extricated by the union of dephlogiſticated and inflammable air to be equal to that extricated by the burning of phoſphorus, we ſhall find, that the union of 120 grains of inflammable air with 576 grains of dephlogiſticated air, extricates 9265° of heat.

18. In the experiment on the deflagration of nitre with charcoal, by Meſſ. LAVOISIER and DE LA PLACE, an ounce of nitre and one third of an ounce of charcoal melted twelve ounces of ice. Suppoſing the ounce of nitre to have produced half an ounce of dephlogiſticated air, it ought to have conſumed 0,1507 ounces of charcoal, and ſhould have melted 14,773 ounces of ice; and I ſuppoſe it fell ſhort of its effect by the heat not being ſufficiently intenſe to decompoſe the nitre perfectly.

19. By the above gentlemen's experiment an ounce of charcoal required for its combuſtion 3,3167 ounces of dephlogiſticated air, and produced 3,6715 ounces of fixed air; therefore there was united to each ounce of air, when changed into fixed air, 61,5 grains of phlogiſton, and 3988° of heat were extracted. *It appears by theſe facts, that the union of phlogiſton, in different proportions, with dephlogiſticated air, does not extricate proportional quantities of heat.* For the addition of 61,5 grains produces 3988°. and the union of 120 grains produces 9265°. This difference may ariſe from a miſtake in ſuppoſing the ſpecific gravity of the inflammable air Dr. PRIESTLEY employed to have been only $\frac{1}{7,6}$ of that of dephlogiſticated air; for if it be ſuppoſed that its ſpecific gravity was a little more than $\frac{1}{7}$ of that of the dephlogiſticated air, then equal additions of phlogiſton would

have

have produced equal quantities of heat*: this matter ſhould therefore be put to the teſt of experiment, by deflagrating dephlogiſticated air with inflammable air of a known ſpecific gravity, or by finding how much dephlogiſticated air is neceſſary for the combuſtion of an ounce of ſulphur, the quantity of phlogiſton in which has been accurately determined by Mr. KIRWAN; or by finding the quantity of phlogiſton in phoſphorus, the quantity of dephlogiſticated air neceſſary for its decompoſition being known from Meſſ. LAVOISIER and DE LA PLACE's experiments.

On conſidering theſe latter gentlemen's experiments on the combuſtion of charcoal, a difficulty ariſes, to know what became of the remainder of the ounce of charcoal; for the dephlogiſticated air, in becoming fixed air, gained only the weight of 0,3548, or about $\frac{1}{3}$ of an ounce; about $\frac{2}{3}$ of an ounce are therefore unaccounted for. The weight of the aſhes of an ounce of charcoal is very inconſiderable; and, by ſome experiments of Dr. PRIESTLEY's, charcoal, when freed from fixed air, and other air which it imbibes from the atmoſphere, is almoſt wholly convertible into phlogiſton. The cauſe of this apparent loſs of matter, I doubt not, theſe gentlemen can explain ſatisfactorily, and very probably in ſuch a manner as will throw other lights on the ſubject.

* Or it may ariſe from my being miſtaken, in ſuppoſing that the ſame quantity of heat is diſengaged by the union of dephlogiſticated air with phlogiſton, in the form of inflammable air, as is by its union with the phlogiſton of phoſphorus or ſulphur; and there appears to be ſome reaſon why there ſhould not; becauſe in theſe latter caſes the water, being united to the acids, cannot retain ſo much elementary heat as it can do when left in the form of pure water, which is the caſe when the inflammable air is uſed.

It is alſo worthy of enquiry, whether all the amazing quantity of heat let looſe in theſe experiments was contained in the dephlogiſticated air; or whether the greateſt portion of it was not contained in the phlogiſton or inflammable air. If it was all contained in the dephlogiſticated air, "*the general rule is not* "*fact, that elaſtic fluids are enlarged in their dimenſions in propor-*" *tion to the quantity of heat they contain*;" becauſe then, inflammable air, which is ten times the bulk of dephlogiſticated air, muſt be ſuppoſed to contain no heat at all; "and it is known, "from ſome experiments of my friend Dr. BLACK's, and ſome "of my own, that the ſteam of boiling water, whoſe latent "and ſenſible heat are only 1100°, reckoning from 60°, or tem-"perate, is more than twice the bulk of an equal weight of "dephlogiſticated air." It ſeems, however, reaſonable to ſuppoſe, that the greater quantity of heat ſhould be contained in the rarer fluid.

It may be alledged, that in proportion to the quantity of phlogiſton that is contained in any fluid, the quantity of heat is leſſened. But if we reaſon by analogy, the attraction of the particles of matter to one another in other caſes is increaſed by phlogiſton, and "bodies are thereby rendered ſpecifically "heavier;" and we know of no other ſubſtance beſides heat which can be ſuppoſed to ſeparate the particles of inflammable air, and to endow it with ſo very great an elaſtic power, and ſo ſmall a ſpecific gravity. On the other hand, if a great quantity of elementary heat be allowed to be contained in inflammable air, on account of its bulk, the ſame reaſoning cannot hold good in reſpect to the phlogiſton of phoſphorus, ſulphur, charcoal, &c. But all theſe ſubſtances contain other matters beſides phlogiſton and heat. The acids in the ſulphur

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and phosphorus, and the alkali and earth in charcoal, may attract the phlogiston so powerfully that the heat they contain may not be able to overcome the adhesion of their particles, until, by the effect of external heat, they are once removed to such a distance from one another as to be out of the sphere of that kind of attraction *.

If it be found to be a constant fact, that equal additions of phlogiston to dephlogisticated air do not extricate equal quantities of heat, that may afford the means of finding the quantities of heat contained in phlogiston and dephlogisticated air respectively, and solve the problem.

Many other ideas on these subjects present themselves; but I am not bold enough to trouble you, or the public, with any speculations, but such as I think are supported by uncontroverted facts.

I must therefore bring this long letter to a conclusion, and leave to others the future prosecution of a subject which, however engaging, my necessary avocations prevent me from pursuing. I cannot however conclude, without acknowledging my obligations to Dr. PRIESTLEY, who has given me every information and assistance in his power, in the course of my enquiries, with that candour and liberality of sentiment which distinguish his character.

I return you my thanks for the obliging attention you have paid to this hypothesis; and remain, with much esteem, &c.

JAMES WATT.

* On the whole, this question seems to involve so many difficulties, that it cannot be cleared up without many new experiments.

XXVI. *Sequel to the Thoughts on the conſtituent Parts of Water and Dephlogiſticated Air. In a ſubſequent Letter from Mr.* James Watt, *Engineer, to Mr.* De Luc, *F. R. S.*

Read May 6, 1784.

DEAR SIR, Birmingham, April 30, 1784.

ON re-conſidering the ſubject of my letter to you of the 26th of November laſt, I think it neceſſary to reſume the ſubject, in order to mention ſome neceſſary cautions to thoſe who may chuſe to repeat the experiments mentioned there, and to point out ſome circumſtances that may cauſe variations in the reſults.

In experiments where the dephlogiſticated air is to be diſtilled from common or cubic nitre, theſe ſalts ſhould be purified as perfectly as poſſible, both from other ſalts and from phlogiſtic matter of any kind; otherwiſe they will produce ſome nitrous air, or yellow fumes, which will leſſen the quantity, and, perhaps, debaſe the quality of the dephlogiſticated air. If the nitre is perfectly pure, no yellow fumes are perceptible, until the alkaline part begins to act upon the glaſs of the retort, and even then they are very ſlightly yellow.

When earthen retorts are uſed, and a large quantity of air is drawn from the nitre, it acts very much upon the retort, diſſolves a great part of it, and becomes very alkaline, retaining only a ſmall part of its acid, at leaſt only a ſmall part which

can be made appear in any of the known forms of that acid; and unleſs retorts can be obtained of a true apyrous and compact porcelain, I ſhould prefer glaſs retorts, properly coated, for making experiments for the preſent purpoſe.

In ſome of my experiments the nitre was left in the retort placed in a furnace, ſo that it took an hour or more to cool. In theſe caſes there was always a deficience of the acid part, which ſeemed, from ſome appearances on the coating, either to have penetrated the hot and ſoft glaſs, by paſſing from particle to particle, or to have eſcaped by ſmall cracks which happened in the retort during the cooling. There was the leaſt deficience of the acid when the diſtillation was performed as quickly as was practicable, and the retort was removed from the fire immediately after the operation was finiſhed. In order to ſhorten the duration of the experiment, and conſequently to leſſen the action of the nitre on the retort, it is adviſable not to diſtil above 50 ounce meaſures of dephlogiſticated air from an ounce of nitre. The experiment has ſucceeded beſt when the retort was placed in a charcoal fire in a chafing-diſh or open furnace; becauſe it is eaſy in that caſe to ſtop the operation, and to withdraw the retort at the proper period.

When the dephlogiſticated air is diſtilled from the nitre of mercury, the ſolution ſhould be performed in the retort itſelf, and the nitrous air produced by the ſolution ſhould be caught in a proper receiver, and decompoſed by the gradual admiſſion of common air through water; and the water, which thus becomes impregnated with the acid of the nitrous air, ſhould be added after the proceſs to the water through which the dephlogiſticated air has paſſed. When the ſolution ceaſes to give any more nitrous air, the point of the tube of the retort ſhould be raiſed out of the water; otherwiſe, by the condenſation of the

watery and acid vapours which follow, a partial exhauſtion will take place, and the receiving water will riſe up into the retort and break it, or at leaſt ſpoil the experiment. A common receiver, ſuch as is uſed in diſtilling ſpirit of nitre, ſhould be applied, with a little water in it, to receive the acid ſteam; and it ſhould be kept as cool as can conveniently be done, as theſe fumes are very volatile. This receiver ſhould remain as long as the fumes are colourleſs; but when they appear, in the neck of the retort, of a yellow colour, it is a mark that the mercurial nitre will immediately produce dephlogiſticated air; the receiver ſhould then be withdrawn, and an apparatus placed to receive the air. The reſt of the proceſs has been ſufficiently explained in my former letter.

The phlogiſticated nitrous acid, ſaturated by an alkali, will not cryſtallize; and, if expoſed to evaporation, even in the heat of the air, will become alkaline again, which ſhews the weakneſs of its affinity with alkalies when diſſolved in water *; a farther proof of which is, that it is expelled from them by all the acids, even by vinegar (which fact has been obſerved by Mr. SCHEELE). I have obſerved, that litmus is no teſt of the ſaturation of this acid by alkalies; for the infuſion of litmus added to ſuch a mixture will turn red, when the liquor appears to be highly alkaline, by its turning the infuſions of violets, roſe leaves, and moſt other red juices, green. This does not proceed from the infuſion of litmus being more ſenſible to the preſence of acids than other teſts; for I have lately diſcovered a teſt liquor (the preparation of which I mean to publiſh ſoon) which is more ſenſible to the preſence of acids

* You have informed me, that Mr. CAVENDISH has alſo obſerved this fact; and that he has mentioned it in a paper lately read before the Royal Society; but I had obſerved the fact previous to my knowledge of his paper.

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than litmus is; but which turns green in the ſame ſolution of phlogiſticated nitre that turns litmus red.

The unavoidable little accidents which have attended theſe experiments, and which tend to render their reſults dubious, have prevented me from relying on them as *full* proofs of the poſition that no acid enters into the compoſition of dephlogiſticated air; though they give great probability to the ſuppoſition. I have, therefore, explained the whole of the hypotheſis and experiments with the diffidence which ought to accompany every attempt to account for the phænomena of nature on other principles than thoſe which are commonly received by philoſophers in general. And in purſuance of the ſame motives it is proper to mention, that the alkali employed to ſaturate the phlogiſticated nitrous acid, was always that of tartar which is partly mild; and I have not examined whether highly phlogiſticated nitrous acid can perfectly expel fixed air from an alkali, though I know no fact which proves the contrary. It ſhould alſo be examined, whether the ſame quantity of real nitrous acid is requiſite to ſaturate a given quantity of alkali, when the acid is phlogiſticated, as is neceſſary when it is dephlogiſticated.

As I am informed that you have done me the honour to communicate my former letter on this ſubject to the Royal Society, I ſhall be obliged to you to do me the ſame favour in reſpect to the preſent letter, if you judge that it merits it.

I remain, &c.

JAMES WATT.

XXVII. *An Attempt to compare and connect the Thermometer for ſtrong Fire, deſcribed in Vol. LXXII. of the Philoſophical Tranſactions, with the common Mercurial Ones.* *By Mr.* Joſiah Wedgwood, *F. R. S. Potter to Her Majeſty.*

Read May 13, 1784.

THIS thermometer, which I had the honour of laying before the Royal Society in May 1782, has now been found, from extenſive experience, both in my manufactories and experimental enquiries, to anſwer the expectations I had conceived of it as a meaſure of all degrees of common fire above ignition: but at preſent it ſtands in a detached ſtate, not connected with any other, as it does not begin to take place till the heat is too great to be meaſured or ſupported by mercurial ones.

What is now therefore wanting, to give us clear ideas of the value of its degrees, is, to connect it with one which long uſe has rendered familiar to us; ſo that if the ſcale of the common thermometer be continued indefinitely upwards as a ſtandard, the diviſions of mine may be reduced to that ſcale, and we may thus have the whole range of the degrees of heat brought into one uniform ſeries, expreſſed in one language, and comparable in every part, from the loweſt that have hitherto been produced by any artificial freezing mixtures, up to the higheſt that can be obtained in our furnaces, or that the materials of our furnaces and veſſels can ſupport.

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The hope of attaining this desirable and important object gave rise to the experiments which I have now the honour of communicating. How far I may have succeeded, or whether the means employed were adequate to the end proposed, is, with all deference, submitted to this illustrious Society.

This attempt is founded upon the construction and application of an intermediate measure, which takes in both the heats that are measurable by the mercurial thermometer, and a sufficient number of those that come within the province of mine to connect the two together; the manner of doing which will be apparent from the three first figures (tab. XIV.); wherein F represents FAHRENHEIT's thermometer, with a continuation of the scale; W my thermometer; and M the intermediate measure divided into any number of equal parts at pleasure.

For if the heat of boiling water, or 212 degrees of FAHRENHEIT, be communicated to M, and its measure upon M marked, as at *a*; and if the heat of boiling mercury, or 600° of FAHRENHEIT, be also communicated to M, and marked as at *b*; it is plain, that the number of degrees upon M between *a* and *b* will be equal to the interval between 212 and 600, that is, to 388° upon FAHRENHEIT.

In like manner, upon exposing M to two different heats above ignition along with my thermometer pieces, if a certain degree of my scale be found to correspond with the point *d*, and another degree of mine with the point *c*; then the interval between those two degrees upon mine must be equal to the interval *dc*; and how many of FAHRENHEIT's that interval is equivalent to will be known from the preceding comparison. Thus we can find the number of FAHRENHEIT's degrees contained in any given extent of mine, and the degree of FAHRENHEIT's with which a given point of mine coincides; whence

whence either ſcale is eaſily reducible to the other through their whole range, whether we ſuppoſe FAHRENHEIT's continued upwards, or mine downwards.

For obtaining the intermediate thermometer different means were thought of; but the only principle which, upon attentive conſideration, afforded any proſpect of ſucceſs, was the *expanſion of metals*. This therefore was adopted, and among different methods of meaſuring that expanſion, which either occurred to myſelf, or which I can find to have been practiſed by others, there is no one which promiſes either ſo great accuracy, or convenience in uſe, as a gage like that by which the thermometer pieces are meaſured: the utility of this gage had now been confirmed to me by experience, and the machines and long rods, which have been employed for meaſuring expanſions on other occaſions, were abſolutely inadmiſſible here, on account of the inſuperable difficulties of performing nice operations of this kind in a red heat, and of communicating a perfectly equal heat through any conſiderable extent.

To give a clearer idea of this ſpecies of gage, which, ſimple as it is, I am informed has been miſunderſtood by ſome of the readers of my former paper, a repreſentation of one uſed on the preſent occaſion is annexed in fig. 4. where ABCD is a ſmooth flat plate; and EF and GH two rulers or flat pieces, a quarter of an inch thick, fixed flat upon the plate, with the ſides that are towards one another made perfectly true, a little further aſunder at one end EG than at the other end FH; thus they include between them a long converging canal, which is divided on one ſide into a number of ſmall equal parts, and which may be conſidered as performing the offices both of the tube and ſcale of the common thermometer. It is obvious, that if a body, ſo adjuſted as to fit exactly at the wider end of this

this canal, be afterwards diminiſhed in its bulk by fire, as the thermometer pieces are, it will then paſs further in the canal, and more and more ſo according as the diminution is greater; and converſely, that if a body, ſo adjuſted as to paſs on to the narrow end, be afterwards expanded by fire, as is the caſe with metals, and applied in that expanded ſtate to the ſcale, it will not paſs ſo far; and that the diviſions on the ſide will be the meaſures of the expanſions of the one, as of the contractions of the other, reckoning in both caſes from that point to which the body was adjuſted at firſt.

I is the body whoſe alteration of bulk is thus to be meaſured, which, in the preſent inſtance, is a piece of fine ſilver: this is to be gently puſhed or ſlid along, towards the end FH, till it is ſtopped by the converging ſides of the canal.

K is a little veſſel formed in the gage for this particular ſeries of experiments, the uſe of which will appear hereafter.

The *contraction*, which the thermometer pieces receive from fire, is a permanent effect, not variable by an abatement of the heat, and which accordingly is meaſured commodiouſly and at leiſure, when the pieces are grown cold. But the *expanſion* of bodies is only temporary, continuing no longer than the heat does that produced it; and therefore its quantity, at any particular degree of heat, muſt be meaſured in the moment while that heat ſubſiſts. And further, if the heated piece was applied to the cold gage, the piece would be deprived of a part of its heat on the firſt contact; and as the gage receives ſome degree of expanſion from heat as well as the piece, it is plain that in this caſe the piece would be diminiſhed in its bulk, and the gage enlarged, before the meaſurement could be taken. It is therefore neceſſary that both of them be heated to an exact equality; and in that ſtate we can meaſure, not indeed the *true expanſion of*

either, but the *excess* of the expansion of one above that of the other, which is sufficient for the present purpose, as we want only an uniform and graduated effect of fire, and it is totally immaterial whether that effect be the absolute expansion of one or the other body, or the difference of the two, provided only that its *quantity* be sufficient to admit of nice measurement.

Some difficulties occurred with respect to the choice of a proper *matter* for the gage; the essential requisites of which are, to have but little expansibility, and to bear the necessary fires without injury. All the metals, except gold and silver, would calcine in the fire: those two are indeed free from that objection, and accordingly it is of the most expansible of them that the piece is made; but if the gage also was made of the same, the measure itself would expand just as much as the body to be measured, and no expansion at all would be sensible; and though the gage was made of one of those metals, and the piece of the other, the difference between their expansions would be too small to give any satisfactory results, as more than two-thirds of the real expansion of either would be lost or taken off by the other.

For these reasons I had recourse to earthy compositions, which expand by heat much less than metallic bodies, and bear the necessary degrees of fire without the least injury. I made choice of tobacco-pipe clay, mixed with charcoal in fine powder, in the proportion of three parts of the charcoal to five of the clay by weight. By a free access of air, in the burning by which the gage is prepared for use, the charcoal is consumed, and leaves the clay extremely light and porous; from which circumstance it bears sudden alternations of cold and heat, often requisite in these operations, much better than the clay alone. Another and more important motive for the use of charcoal

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was, that in conſequence of the remarkable poroſity which it produces in the clay, it would probably diminiſh the expanſibility, by occaſioning the maſs to contain, under an equal ſurface, a much leſs quantity of ſolid or expanſible matter. It may be objected to this idea, that the expanſions of metals, in Mr. ELLICOTT's * and Mr. SMEATON's † experiments, do not appear to have any connection at all with their denſities: but the caſes are by no means parallel; for there the compariſon lies between different ſpecies of matter; but here, between one and the ſame matter in different ſtates of compactneſs. If a metal could be treated as clay is in this inſtance, that is, if a large bulk of any foreign matter could be blended with it, and this matter afterwards burnt out, ſo as to leave the metallic particles at the ſame diſtances to which they had been ſeparated by the mixture of it, we may preſume that the metal thus enlarged would not expand ſo much as an equal volume of the ſolid metal. Such at leaſt were the ideas which determined my choice to a compoſition of clay and charcoal powder; and being afterwards deſirous of ſatisfying myſelf whether they had any foundation in fact, I have, ſince the experiments were made, prepared ſome pieces of clay with and without charcoal, and having burnt them in the ſame fire, I ground them at the ſides, to make them both fit exactly to the ſame diviſion near the narrow end of the gage; then, examining their expanſions by equal heats, I found the piece with charcoal to expand only one-third part ſo much as that without; and thus was fully ſatisfied with the compoſition of the gage.

To aſcertain a fixed point on the ſcale for the diviſions to be counted from, the ſilver piece and gage were laid together for

* Phil. Tranſact. vol. XLVII. p. 485.

† Ibid. vol. XLVIII. p. 612.

ſome time in ſpring water, of the temperature of 50° of FAHRENHEIT: the point which the piece went to in this cold ſtate is that marked o near the narrow end of the gage. The adjuſtment is re-examined at the beginning and end of every ſucceeding experiment, leſt the repeated attrition, in ſliding the piece backwards and forwards, ſhould wear off ſo much from the ſurface of this ſoft metal as to occaſion an error in the minute quantities here meaſured.

The apparatus is then expoſed ſucceſſively to different degrees of heat, with the piece lying always in a part of the canal at leaſt as wide as it is expected to fill when expanded, otherwiſe the ſides of the gage would be burſt aſunder by its expanſion, as I experienced in ſome of my firſt trials. When the whole has received any particular degree of heat deſired, the piece is cautiouſly and equably puſhed along, till it is ſtopped by the convergency of the ſides, of which I always find notice given me by the gage itſelf (which is ſmall and light) beginning to move upon the continuance of the impulſe. A flat ſlip of iron, a little narrower than the piece, bent down to a right-angle at one end, and fixed in a long handle at the other, makes a convenient inſtrument for puſhing the piece forward, or drawing it back again, whilſt red-hot: this inſtrument, at every time of uſing, is heated to the ſame degree as the piece itſelf.

The heat of boiling water is taken without difficulty, by keeping the apparatus in boiling water itſelf during a ſufficient ſpace of time for the full heat to be communicated to it. The water I made uſe of was a very fine ſpring water, which on chemical trials appeared very nearly equal in purity to that of rain or ſnow; and I had previouſly ſatisfied myſelf, by trials in the cold, that the gage and piece being wet, or under water, made no difference in the meaſurement. The expanſion of the

ſilver by this heat, that is, by an increaſe of the heat from 50° to 212°, or a period containing 162° of FAHRENHEIT, was juſt 8° of the gage or intermediate thermometer M; whence one of theſe degrees, according to this experiment, contains juſt 20°¼ of FAHRENHEIT's. The operation was many times repeated, and the reſult was always preciſely the ſame.

For the boiling heat of mercury, it was neceſſary to proceed in a different manner; not to convey the heat from the mercury to the inſtrument, but to convey it equally to them both from another body. I made a ſmall veſſel for holding the mercury in the gage itſelf, ſeen at K fig. 4. and more diſtinctly in fig. 5. which is a tranſverſe ſection of the gage through this veſſel. The plate CD, which forms the bottom of the canal, ſerves alſo for the bottom of the veſſel, which is ſituated cloſe to the ſide of the canal, and as near as could be to that part of it, in which both the ſilver piece, and the diviſions required for this particular experiment, are contained. By this arrangement it is preſumed, that all the parts concerned in the operation will receive very nearly an equal heat.

The gage, with ſome mercury in the veſſel, was laid upon a ſmooth and level bed of ſand, on the bottom of an iron muffle kept open at one end; the fire increaſed very gradually till the mercury boiled, and then continued ſteady, ſo as juſt to keep it boiling, for a conſiderable time. The boiling heat of mercury was thus found to be 27°¼ of the intermediate thermometer, which anſwering to an interval of 550° of FAHRENHEIT, makes one degree of this equal to juſt 20° of his; a reſult correſponding even beyond my expectations with that which boiling water had given.

Theſe ſtandard heats of FAHRENHEIT's thermometer are obtained with little difficulty on a common fire; but it is far otherwiſe

otherwiſe with the higher ones in which mine begins to apply; and all the precautions I could take, by uſing a cloſe muffle, ſurrounding it as equally as poſſible with the fuel, varying its poſition with reſpect to the draught of air, &c. proved inſufficient for ſecuring the neceſſary equality of heat even through the ſmall ſpace concerned in theſe experiments. Nor had I any idea, before the diſcovery of this thermometer, of the extreme difficulty, not to ſay impracticability, of obtaining, in common fires, or in common furnaces, an uniform heat through the extent even of a few inches. Incredible as this may appear at firſt ſight, whoever will follow me in the operations I have gone through, placing accurate meaſures of the heat in different parts of one and the ſame veſſel, will ſoon be convinced of its truth, and that he can no otherwiſe expect to communicate with certainty an equal heat to different pieces, than by uſing a fire of ſuch magnitude as to exceed perhaps ſome hundreds of times the bulk of the matters required to be heated.

To ſuch large body of fire, therefore, after many fruitleſs attempts in ſmall furnaces, not a little diſcouraging by the irregularity of their reſults, I at length had recourſe, fitting up for this purpoſe an iron oven, uſed for the burning-on of enamel colours upon earthen ware, about four feet long, by two and a half wide, and three feet high, which is heated by the flame of wood conducted all round it. An iron muffle, four inches wide, two inches and three quarters high, and ten inches long, containing the gage and piece, was placed in the middle of this oven, and the vacancy between them filled up with earthen ware, to increaſe the quantity of ignited matter, and thereby communicate the heat more equably from the oven to the muffle. In ſuch a ſituation of the muffle, in the center

center of an oven more than five hundred times its own capacity, it could not well fail of being heated pretty uniformly, at leaſt through the ſmall ſpace which theſe experiments required; nor have I found any reaſon to ſuſpect that it was not ſo.

The gage being laid flat upon the bottom of the muffle, with the ſilver piece in the canal as before, ſome of the clay thermometer pieces were ſet on end upon the ſilver piece, with that end of each downwards which is marked to go foremoſt in meaſuring it; that is, they were in contact with the ſilver in that part of their ſurface by which their meaſure is afterwards aſcertained. I was led to this precaution by an experiment I had made upon another occaſion, in which a number of thermometer pieces having been ſet upright upon an earthen-ware plate, over a ſmall fire, till the plate became red-hot, all the pieces were found diminiſhed, ſome of them more than two degrees, at the lower ends which reſted upon the plate, whilſt the upper ends were as much enlarged, not having yet paſſed the ſtage of extenſion which, as obſerved in the former paper, always precedes the thermometric diminution: thus we ſee how punctually every part of the piece obeys the heat that acts upon it.

The fire about the oven was ſlowly increaſed for ſome hours, and kept as even and ſteady as poſſible, by an experienced fireman, under my own inſpection. Upon opening a ſmall door, which had been made for introducing the apparatus, and looking in from time to time, it was obſerved, that the muffle, with the adjacent parts of the oven and ware, acquired a viſible redneſs at the ſame time; and in the progreſs of the operation, the eye could not diſtinguiſh the leaſt diſſimilarity in the aſpect of the different parts; whereas in ſmall fires, the difference not only between the two ends of the muffle, but in much leſs diſtances, is ſuch as to ſtrike the eye at once.

When

When the muffle appeared of a low red heat, ſuch as was judged to come fully within the province of my thermometer, it was drawn forward, towards the door of the oven; and its own door being then nimbly opened by an aſſiſtant, I immediately puſhed the ſilver piece as far as it would go. But as the diviſion which it went to could not be diſtinguiſhed in that ignited ſtate, the muffle was lifted out, by means of an iron rod paſſed through two rings made for that purpoſe, with care to keep it ſteady, and avoid any ſhake that might endanger the diſplacing of the ſilver piece.

When grown ſufficiently cold to be examined, I noted the degree of expanſion which the ſilver piece ſtood at, and the degree of heat ſhewn by the thermometer pieces meaſured in their own gage; then returned the whole into the oven as before, and repeated the operation with a ſtronger heat, to obtain another point of correſpondence on the two ſcales.

The firſt was at 2°¼ of my thermometer, which coincided with 66° of the intermediate one; and as each of theſe laſt has been before found to contain 20 of FAHRENHEIT's, the 66 will contain 1320; to which add 50, the degree of his ſcale to which the 0 of the intermediate thermometer was adjuſted, and the ſum, 1370, will be the degree of FAHRENHEIT's corresponding to my 2°¼.

The ſecond point of coincidence was at 6°¼ of mine, and 92° of the intermediate; which 92 being, according to the above proportion, equivalent to 1840 of FAHRENHEIT, add 50 as before to this number, and my 6°¼ is found to fall upon the 1890th degree of FAHRENHEIT.

It appears from hence, that an interval of 4 degrees upon mine is equivalent to an interval of 520° upon his; conſequently 1 of mine to 130 degrees of his; and that the 0 of mine correſponds

to

to his 1077°½. Several other trials were made, which gave results so nearly alike, that I have little apprehension of any material error.

From these data it is easy to reduce either scale to the other through their whole range; and from such reduction it will appear, that an interval of near 480° remains between them, which the intermediate thermometer serves as a measure for; that mine includes an extent of about 32000 of FAHRENHEIT's degrees, or about 54 times as much as that between the freezing and boiling points of mercury, by which mercurial ones are naturally limited; that if the scale of mine be produced downwards, in the same manner as we have supposed FAHRENHEIT's to be produced upwards, for an ideal standard, the freezing point of water would fall nearly on 8° below 0 of mine, and the freezing point of mercury a little below 8°½; and that, therefore, of the extent of now measurable heat, there are about $\frac{1}{10}$ths of a degree of my scale from the freezing of mercury to the freezing of water; 8° from the freezing of water to full ignition; and 160° above this to the highest degree I have hitherto attained.

As we are now enabled to compare not only the higher degrees among themselves, and the lower among themselves, upon their respective scales, but likewise the higher and lower with each other in every stage, it may be proper to take a general view of the whole range of measurable heat, as expressed both in FAHRENHEIT's denominations and in mine; and for this purpose I have drawn up a little table of a few of the principal points that have been ascertained, to shew their mutual relations or proportions to each other: any other points that have been, or hereafter may be, observed, by these or any other known thermometers, may be inserted at pleasure.

	FAHR.	WEDG.
Extremity of the ſcale of my thermometer	32277°	240°
Greateſt heat of my ſmall air-furnace -	21877	160
Caſt iron melts - - - -	17977	130
Greateſt heat of a common ſmith's forge	17327	125
Welding heat of iron, greateſt -	13427	95
——————— leaſt - -	12777	90
Fine gold melts - - -	5237	32
Fine ſilver melts - - -	4717	28
Swediſh copper melts - -	4587	27
Braſs melts - - -	3807	21
Heat by which my enamel colours are burnt on	1857	6
Red-heat fully viſible in day-light -	1077	0
Red-heat fully viſible in the dark -	947	1
Mercury boils - - -	600	$3\frac{673}{1000}$
Water boils - - -	212	$6\frac{618}{1000}$
Vital heat - - -	97	$7\frac{549}{1000}$
Water freezes - - -	32	$8\frac{42}{1000}$
Proof ſpirit freezes - - -	0	$8\frac{239}{1000}$
The point at which mercury congeals, conſequently the limit of mercurial thermometers, - -	about 40	$8\frac{306}{1000}$

To aſſiſt our conceptions of this ſubject, it may be proper to view it in another light, and endeavour to preſent it to the eye; for *numbers*, on a high ſcale, are with difficulty eſtimated and compared by the mind. I have therefore completed the ſcales of which a part is repreſented in fig. 1. and 3. by continuing the ſame equal diviſions, both upwards and downwards, as far as the utmoſt limits of heat that have hitherto been attained and meaſured *.

* Mr. WEDGWOOD preſented this, in the form of a very long roll, to the Society.

In

In a ſcale of heat drawn up in this manner, the comparative extents of the different departments of this grand and univerſal agent are rendered conſpicuous at a ſingle glance of the eye. We ſee at once, for inſtance, how ſmall a portion of it is concerned in animal and vegetable life, and in the ordinary operations of nature. From freezing to vital heat is barely a five-hundredth part of the ſcale; a quantity ſo inconſiderable, relatively to the whole, that in the higher ſtages of ignition, ten times as much might be added or taken away, without the leaſt difference being diſcernible in any of the appearances from which the intenſity of fire has hitherto been judged of. From hence, at the ſame time, we may be convinced of the utility and importance of a phyſical meaſure for theſe higher degrees of heat, and the utter inſufficiency of the common means of diſcriminating and eſtimating their force. I have too often found differences, aſtoniſhing when conſidered as a part of this ſcale, in the heats of my own kilns and ovens, without being perceivable by the workmen at the time, or till the ware was taken out of the kiln.

SINCE the foregoing experiments were made, I have ſeen a very curious Memoir by Meſſ. LAVOISIER and DE LA PLACE, containing a method of meaſuring heat by the quantity of ice which the heated body is capable of liquefying. The application of this important diſcovery, as an intermediate ſtandard meaſure between FAHRENHEIT's thermometer and mine, could not eſcape me, and I immediately ſet about preparing an apparatus, and making the experiments neceſſary for that pur-

poſe; in hopes either of attaining by this method a greater degree of accuracy than I could expect from any other means, or of having what I had already done confirmed by a ſeries of experiments upon a different principle.

But in the proſecution of theſe experiments I have, to my great mortification, hitherto failed of ſucceſs; and I ſhould have contented myſelf for the preſent with ſaying little more than this, if ſome phænomena had not occurred, which appear to me not unworthy of farther inveſtigation.

The authors obſerve, that if ice, cooled to whatever degree below the freezing point, be expoſed to a warmer atmoſphere, it will be brought up to the freezing point through its whole maſs before any part of its ſurface begins to liquefy; and that conſequently ice, beginning to melt on the ſurface, will be always exactly of the ſame temperature, *viz.* at the freezing point; and that if a heated body be incloſed in a hollow ſphere of ſuch ice, the whole of its heat will be taken up in liquefying the ice; ſo that if the ice be defended from external warmth, by ſurrounding it with other ice in a ſeparate veſſel, the weight of the water produced from it will be exactly proportional to the heat which the heated body has loſt; or, in other words, will be a true phyſical meaſure of the heat.

For applying theſe principles in practice, they employ a tin veſſel, divided, by upright concentric partitions, into three compartments, one within another. The innermoſt compartment is a wire cage, for receiving the heated body. The ſecond, ſurrounding this cage, is filled with pounded ice, to be melted by the heat; and the outermoſt is filled alſo with pounded ice, to defend the former from the warmth of the atmoſphere. The firſt of theſe ice compartments terminates at bottom in a ſtem like a funnel, through which the water is conveyed off; and the other ice compartment terminates in a ſeparate canal, for

diſcharging the water into which *that* ice is reduced. As ſoon as the heated body is dropped into the cage, a cover is put on, which goes over both that and the firſt ice compartment; which cover is itſelf a kind of ſhallow veſſel, filled with pounded ice, with holes in the bottom for permitting the water from this ice to paſs into the ſecond compartment, all the liquefaction that happens here, as well as there, being the effect of the heated body only. Over the whole is placed another cover with pounded ice, as a defence from external warmth.

As ſoon as this diſcovery came to my knowledge, on the 23d of February, a thaw having begun three days before, after a froſt which had continued with very little intermiſſion from the 24th of December, I collected a quantity of ice, and ſtored it up in a large caſk in a cellar.

I thought it neceſſary to ſatisfy myſelf in the firſt place, by actual experiment, that ice, how cold ſoever it may be, comes up to the freezing point through its whole maſs before it begins to liquefy on the ſurface. For this purpoſe I cooled a large fragment of ice, by a freezing mixture, to 17° of FAHRENHEIT's thermometer, and then hung it up in a room whoſe temperature was 50°. When it began to drop, it was broken, and ſome of the internal part nimbly pounded and applied to the bulb of a thermometer that was cooled by a freezing mixture below 30°. The thermometer roſe to, and continued at, 32°; being then taken out, and raiſed by warmth to 40°, ſome more of the ſame ice, applied as before to the bulb, ſunk it again to 32°; ſo that no doubt could remain on this ſubject.

Apprehenſive that pounded ice, directed by the authors, might imbibe and retain more or leſs of the water by capillary attraction, according to circumſtances, and thereby occaſion ſome error in the reſults, I thought it neceſſary to ſatisfy myſelf in this reſpect

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alſo by experiment. I therefore pounded ſome ice, and laid it in a conical heap on a plate; and having at hand ſome water, coloured with cochineal, I poured it gently into the plate, at ſome diſtance from the heap: as ſoon as it came in contact with the ice, it roſe haſtily up to the top; and on lifting up the lump, I found that it held the water, ſo taken up, as a ſponge does, and did not drop any part of it till the heat of my hand, as I ſuppoſe, began to liquefy the maſs. On further trials I found, that in pounded ice preſſed into a conical heap, the coloured water roſe, in the ſpace of three minutes, to the height of two inches and a half; and by weighing the water employed, and what remained upon the plate unabſorbed, it appeared, that four ounces of ice had thus taken up, and retained, one ounce of water.

To further aſcertain this abſorbing power, in different circumſtances, more analogous to thoſe of the proceſs itſelf, I preſſed ſix ounces of pounded ice pretty hard into the funnel, having firſt introduced a wooden core in order to leave a proper cavity in the middle: then, taking out the core, and pouring an ounce of water upon the ice, I left the whole for half an hour; at the end of which time the quantity that ran off was only 12 pennyweights and 4 grains, ſo that the ice had retained 7 pennyweights and 20 grains, which is nearly one-twelfth of its own weight, and two-fifths of the weight of the water.

Theſe previous trials determined me, inſtead of uſing pounded ice, to fill a proper veſſel with a ſolid maſs of ice, by means of a freezing mixture, as the froſt was now gone, and then expoſe it to the atmoſphere till the ſurface began to liquefy. The apparatus I fitted up for this purpoſe was made of earthen ware well glazed, and is repreſented in fig. 6. (tab. XV.).

A, is

A, is a large funnel, filled with a ſolid maſs of ice. B, a cavity in the middle of this ice, formed, part of the way, by ſcraping with a knife, and for the remaining part, by boring with a hot iron wire. C, one of my thermometer pieces, which ſerves for the heated body, and reſts upon a coil of braſs wire: it had previouſly been burnt with ſtrong fire, that there might be no danger of its ſuffering any further diminution of its bulk by being heated again for theſe experiments. D, a cork ſtopper in the orifice of the funnel. E, the exterior veſſel, having the ſpace between its ſides and the included funnel A, filled with pounded ice, as a defence to the ice in the funnel. F, a cover for this exterior veſſel, filled with pounded ice for the ſame purpoſe. G, a cover for the funnel, filled alſo with pounded ice, with perforations in the bottom for allowing the water from this ice to paſs down into the funnel.

The thermometer piece was heated in boiling water, taken up with a pair of ſmall tongs equally heated, dropped inſtantly into the cavity B, and the covers put on as expeditiouſly as poſſible; the bottom of the funnel being previouſly corked, that the water might be detained till it ſhould part with all its heat, and likewiſe to prevent the water from the other ice, which ran down on the outſide of the funnel, from mingling with it.

After ſtanding about ten minutes, the funnel was taken out, wiped dry, and uncorked over a weighed cup: the water that ran out weighed 22 grains. Thinking this quantity too ſmall, as the piece weighed 72 grains, I repeated the experiment, and kept the piece longer in the funnel; but the water this time weighed only 12 grains. Being much diſſatisfied with this reſult, I made a third trial, continuing the piece much longer in the cavity; but the quantity of water was now ſtill leſs, not

amounting to quite three drops; and, to my great ſurpriſe, I found the piece frozen to the ice, ſo as not to be eaſily got off, though all the ice employed was, at the beginning of the experiment, in a thawing ſtate.

I had prepared the apparatus for taking the boiling heat of mercury; but being entirely diſcouraged by theſe very unequal reſults, I gave that up, for the preſent at leaſt, and heating the piece to 6° of my thermometer, turned it nimbly out of the caſe in which it was heated into the cavity, throwing ſome fragments of ice over it. In about half an hour, I drew off the water, which amounted to 11 pennyweights; then ſtopping the funnel again, and replacing the covers, I left the whole about ſeven hours.

At the end of that time, I found a conſiderable quantity of water in the funnel: the melting of the ice had produced a cavity between it and the ſides, great part of the way down, which, as well as that in the middle, was nearly full. The water nevertheleſs ran out ſo ſlowly, that I apprehended ſomething had ſtopped the narrow end of the funnel, but the true cauſe became afterwards apparent upon examining the ſtate of the ice. The fragments which I had thrown over the thermometer piece were frozen entirely together, and in ſuch a form as they could not have aſſumed without freſh water ſuperadded and frozen upon them, for the cavities between them were partly filled with new ice. I endeavoured to take the ice out with my fingers, but in vain; and it was with ſome difficulty I could force it aſunder even with a pointed knife, to get at the thermometer piece. When that was got out, great part of the coiled wire was found enveloped in new ice. The paſſage through the ice to the ſtem of the funnel, which I had made pretty wide with a thick iron wire red-hot, was ſo nearly cloſed

up,

up, that the ſlow draining off of the water was now ſufficiently accounted for, and indeed this draining was the only apparent mark of any paſſage at all. On taking the ice out of the funnel, and breaking it to examine this canal, I found it almoſt entirely filled up with ice projecting from the ſolid maſs in cryſtalline forms, ſimilar in appearance to the cryſtals we often meet with in the cavities of flints and quarzoſe ſtones.

If, after all theſe circumſtances, any doubt could have remained of the ice in queſtion being a new production, a fact which I now obſerved muſt have removed all ſuſpicion. I found a coating of ice, of conſiderable extent and perfectly tranſparent, about a tenth of an inch in thickneſs, upon the outſide of the funnel, and on a part of it which was not in contact with the ſurrounding ice, for that was melted to the diſtance of an inch from it.

Some of the ice being ſcraped off from the inſide of the funnel, and applied to the bulb of the thermometer, the mercury ſunk from 50° to 32°, and continued at that point till the ice was melted; after which, the water being poured off, it roſe in a little time to 47°.

Aſtoniſhed at theſe appearances, of the water freezing after it had been melted, though ſurrounded with ice in a melting ſtate, and in an atmoſphere about 50°, where no part of the apparatus or materials could be ſuppoſed to be lower than the freezing point, I ſuſpected at firſt that ſome of the ſalt of the freezing mixture might have got into the water, and that this, in diſſolving, might perhaps abſorb, from the parts contiguous to it, a greater proportion of heat than the ice of pure water does. But the water betrayed nothing ſaline to the taſte, and I had applied the freezing mixture with my own hands with great care, to prevent any of it being mixed with the water.

To remove all doubts, however, upon this point, I purpoſed repeating the experiment with ſome pieces of the ice I had ſtored up in the cellar, to ſee if this would congeal, after thawing, in the ſame manner. But going to fetch the ice, and examining it in the caſk in which it was kept, I was perfectly ſatisfied with the appearances I found there; for though much of it was melted, yet the fragments were frozen together, ſo that it was with difficulty I could break or get out any pieces of it with an iron ſpade; and, when ſo broken, it had the appearance of *breccia* marble or plum-pudding ſtone, for the fragments had been broken and rammed into the caſk with an iron mall.

A porcelain cup being laid upon ſome of this ice about half an hour, in a room whoſe temperature was 50°, it was found pretty firmly adhering, and when pulled off, the ice exhibited an exact impreſſion of the fluted part of the cup which it had been in contact with; ſo that the ice muſt neceſſarily have liquefied firſt, and afterwards congealed again. This was repeated ſeveral times, with the ſame event. Fragments of the ice were likewiſe applied to one another, to ſponges, to pieces of flannel and of linen cloth, both moiſt and dry: all theſe, in a few ſeconds, began to cohere, and in about a minute were frozen ſo as to require ſome force to ſeparate them. After ſtanding an hour, the cohesion was ſo firm, that on pulling away the fragments of ice from the woollen and ſponge, they tore off with them that part of the ſurface which they were in contact with, though at the ſame time both the ſponge and flannel were filled with water which that very ice had produced.

To make ſome eſtimate of the force of the congelation, which was ſtronger on the two bodies laſt mentioned than on

linen, I applied a piece of ice to a piece of dry flannel which weighed two pennyweights and a half, and ſurrounded them with other ice. After lying together three quarters of an hour, taking the piece of ice in my hand and hooking the flannel to a ſcale, I found a weight of five ounces to be neceſſary for pulling it off, and yet ſo much of the ice had liquefied as to increaſe the weight of the flannel above 12 pennyweights. I then weighed the piece of ice, put them together again, and four hours after found them frozen ſo firmly as to require 78 ounces for their ſeparation, although, from 42 pennyweights of the ice, 15 more had melted off: the ſurface of contact was at this time nearly a ſquare inch. I continued them again together for ſeven hours; but they now bore only 62 ounces, the ice being diminiſhed to 14 pennyweights, and the ſurface of contact reduced to about ſix-tenths of a ſquare inch.

Having ſeen before that pounded ice abſorbs water in very conſiderable quantity, I ſuſpected that ſomething of the ſame kind might take place even with entire maſſes; and experiment ſoon convinced me, that even apparently ſolid pieces of ice will imbibe water, ſlower or quicker according to its ſtage of decay. I have repeatedly heated ſome of my thermometer pieces, and laid them upon ice, in which they made cavities of conſiderable depth, but the water was always abſorbed, ſometimes as faſt as it was produced, leaving both the piece and the cavity dry.

Thus, though I cannot ſufficiently expreſs how much I admire the diſcovery that gave riſe to theſe experiments, I have nevertheleſs to lament my not being able to avail myſelf of it at preſent for the purpoſe I wiſhed to apply it to.

That in my experiments the two ſeemingly oppoſite proceſſes of nature, congelation and liquefaction, went on together, at

the ſame inſtant, in the ſame veſſel, and even in the ſame fragment of ice, is a fact of which I have the fulleſt evidence that my ſenſes can give me; and I ſhall take the liberty of ſuggeſting a few hints, which may tend perhaps to elucidate their cauſe, and to ſhew that they are not ſo incompatible as at firſt ſight they appear to be.

It occurred to me at firſt, that water highly attenuated and divided, as when reduced into vapour, may freeze with a leſs degree of cold than water in its aggregate or groſſer form; hence hoar-froſt is obſerved upon graſs, trees, &c. at times when there is no appearance of ice upon water, and when the thermometer is above the freezing point*. BOERHAAVE, I find, in his elaborate theory of fire, aſſigns 33° as the freezing point of vapour, and even of water when divided only by being imbibed in a linen cloth.

* I am aware, that experiments and obſervations of this kind are not fully deciſive; that the atmoſphere may, in certain circumſtances, be much warmer or colder than the earth and waters, which, in virtue of their denſity, are far more retentive of the temperature they have once received, and leſs ſuſceptible of tranſient impreſſions; that even inſenſible undulations of water, from the ſlighteſt motion of the air, by bringing up warmer ſurfaces from below, may prove a further impediment to the freezing; and, therefore, that the degree of cold, which is ſufficient to produce hoar-froſt, may poſſibly, if continued long enough, be ſufficient alſo to produce ice. I am not acquainted with any ſatisfactory experiments or obſervations yet made upon the ſubject; nor do I advance the principle as a certain, but as a probable one, which occurred to me at the moment, which is countenanced by general obſervation, and conſentaneous to many known facts; for there are numerous inſtances of bodies, in an extreme ſtate of diviſion, yielding eaſily to chemical agents which, before ſuch diviſion, they entirely reſiſt: thus ſome precipitates, in the very ſubtile ſtate in which they are at firſt extricated from their diſſolvents, are re-diſſolved by other menſtrua, which, after their concretion into ſenſible moleculæ, have no action upon them at all.

Now, as the atmoſphere abounds with watery vapour, or water diſſolved and chemically combined, and muſt be particularly loaded with it in the neighbourhood of melting ice; as the heated body introduced into the funnel muſt neceſſarily convert a portion of the ice or water there into vapour; and as ice is known to melt as ſoon as the heat begins to exceed 32°, or nearly one degree lower than the freezing point of vapour; I think we may from hence deduce, pretty ſatisfactorily, all the phænomena I have obſerved. For it naturally follows from theſe principles, that vapour may freeze where ice is melting; that the vapour may congeal even upon the ſurface of the melting ice itſelf; and that the heat which (agreeably to the ingenious theory of Dr. BLACK) it emits in freezing, may contribute to the further liquefaction of that very ice upon which the new congelation is formed.

I would further obſerve, that the freezing of water is attended with plentiful evaporation in a cloſe as well as an open veſſel, the vapour in the former condenſing into drops on the under ſide of the cover, which either continue in the form of water, or aſſume that of ice or a kind of ſnow, according to circumſtances *; which evaporation may perhaps be attributed to the heat that was combined with the water, at this moment rapidly making its eſcape, and carrying part of the aqueous fluid off with it. We are hence furniſhed with a freſh and continual ſource of vapour as well as of heat; ſo that the proceſſes of liquefaction and congelation may go on uninterruptedly together, and even neceſſarily accompany one another, although, as the freezing muſt be in an under proportion to the melting, the whole of the ice muſt ultimately be conſumed.

* See Mr. BARON's paper on this ſubject, in the Memoires of the Academy of Sciences at Paris for the year 1753.

In

In the remarkable inſtance of the coating of ice on the outſide of the throat of the funnel, there are ſome other circumſtances which it may be proper to take notice of. Neither the cover of the outer veſſel, nor the aperture in its bottom which the ſtem of the funnel paſſed through, were air-tight, and the melting of the ſurrounding ice had left a vacancy of about an inch round that part of the funnel on which the cruſt had formed. As there was, therefore, a paſſage for air through the veſſel, a circulation of it would probably take place: the cold and denſe air in the veſſel would deſcend into the rarer air of the room then about 50°, and be replaced by air from above. The effect of this circulation and ſudden refrigeration of the air will be a condenſation of part of the moiſture it contains upon the bodies it is in contact with: the throat of the funnel, being one of thoſe bodies, muſt receive its ſhare; and the degree of cold in which the ice thaws being ſuppoſed ſufficient for the freezing of this moiſt vapour, the contact, condenſation, and freezing, may happen at the ſame inſtant.

The ſame principles apply to every inſtance of congelation that took place in theſe experiments; and a recollection of particulars which paſſed under my own eye convinces me, that the congelation was ſtrongeſt in thoſe circumſtances where vapour was moſt abundant, and on thoſe bodies which, from their natural or mechanic ſtructure, were capacious of the greateſt quantity of it; ſtronger, for inſtance, on ſponge than on woollen, ſtronger on this than on the cloſer texture of linen, and far ſtronger on all theſe than on the compact ſurface of porcelain.

If, nevertheleſs, the principle I have aſſumed (that water highly attenuated will congeal with a leſs degree of cold than water in the maſs) ſhould not be admitted; another has above

been

been hinted at, which experiments have decidedly eſtabliſhed, from which the phænomena may perhaps be equally accounted for, and which, even though the other alſo is received, muſt be ſuppoſed to concur for ſome part of the effect; I mean, that *evaporation produces cold*; both vapour and ſteam carrying off ſome proportion of heat from the body which produces them. If, therefore, evaporation be made to take place upon the ſurface of ice, the contiguous ice will thereby be rendered colder; and as it is already at the freezing point, the ſmalleſt increaſe of cold will be ſufficient for freſh congelation. It ſeems to be on this principle that the formation of ice is effected in the Eaſt Indies, by expoſing water to a ſerene air, at the coldeſt ſeaſon of the year, in ſhallow porous earthen veſſels: part of the water tranſudes through the veſſel, and evaporating from the outſide, the remainder in the veſſel becomes cold enough to freeze; the warmth of the earth being at the ſame time intercepted by the veſſels being placed upon bodies little diſpoſed to conduct heat *. If ice is thus producible in a climate where natural ice is never ſeen, we need not wonder that congelation ſhould take place where the ſame principle operates amidſt actual ice.

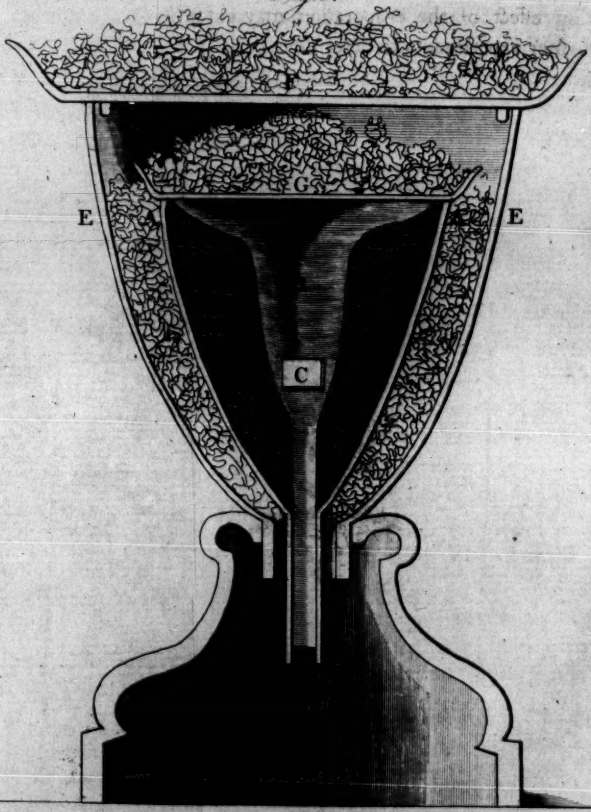
It has been obſerved above, that the heat emitted by the congealing vapour probably unites with and liquefies contiguous portions of ice; but whether the whole, either of the heat ſo emitted, or of that originally introduced into the funnel, is thus taken up; how often it may unite with other portions of ice, and be driven out from other new congelations; whether there exiſts any difference in its chemical affinity or

* See a deſcription of this proceſs in the Philoſophical Tranſactions, vol. LXV. p. 253.

elective

elective attraction to water in different states and the contiguous bodies; whether part of it may not ultimately escape, without performing the office expected from it upon the ice; and to what distance from the evaporating surface the refrigerating effect of the evaporation may extend; must be left for further experiments to determine.

Fig. 6.



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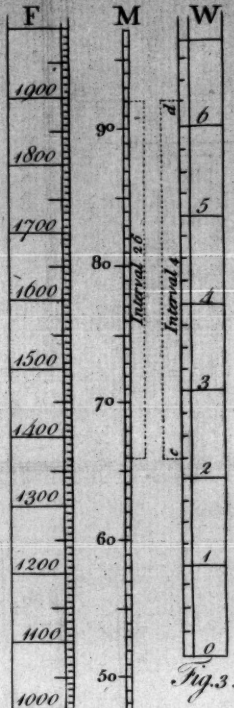
F
M
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1900
1800
1700
1600
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1200
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200
100
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90
80
70
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30
20
10
0
Interval 3.88.
Interval 19½
Interval 2.6
Interval 4
6
5
4
3
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1
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Fig. 1.
Fig. 2.
Fig. 3.

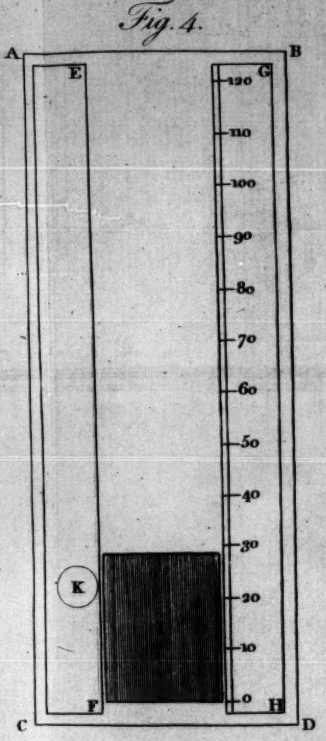
Fig. 4.
A
B
E
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110
100
90
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70
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40
30
20
10
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K
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C
D

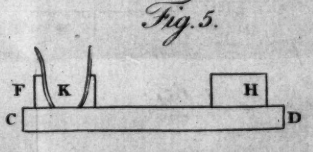
Fig. 5.
F
K
H
C
D

XXVIII. *On the Summation of Series, whoſe general Term is a determinate Function of z the Diſtance from the firſt Term of the Series.* *By* Edward Waring, *M. D.* Lucaſian *Profeſſor of the Mathematics at* Cambridge, *and Fellow of the Societies of* London *and* Bononia.

Read May 20, 1784.

PROBLEM.

THE ſum S *being given, to find a ſeries of which it is the ſum.*

1. Reduce the ſum S into a converging ſeries, proceeding according to the dimenſions of any ſmall quantities, and it is done. For example: let any algebraical function S of an unknown or ſmall quantity x be aſſumed, reduce it into a converging ſeries proceeding according to the dimenſions of x, and there reſults a ſeries whoſe ſum is S. 2. Let A, B, C, &c. be algebraical functions of x; reduce the $\int A\dot{x}$, $\int B\dot{x}$, $\int C\dot{x}$, &c. into a converging ſeries, proceeding according to the dimenſions of x, and the problem is done.

It is always neceſſary to find the values of x, between which the abovementioned ſerieſes converge. Reduce the algebraical function S in the firſt example, and the algebraical functions A, B, C, &c. in the ſecond into their loweſt terms; and in ſuch a manner, that the quantities contained in the numerator and denominator may have no denominator: make the deno-

minator in the first example, and the denominator in the second, and every distinct irrational quantity contained in them respectively $=0$; and also every distinct irrational quantity contained in the numerators $=0$. Suppose α the least root affirmative or negative (but not $=0$) of the abovementioned resulting equations; then a series ascending according to the dimensions of x will always converge, if the value of x is contained between α and $-\alpha$; but if x be greater than α or $-\alpha$, the abovementioned series will diverge. Let π be the greatest root of the abovementioned resulting equations; then a series descending according to the reciprocal dimensions of x will converge, if x be greater than $\pm\pi$; but, if less, not. When impossible roots $a \pm b\sqrt{-1}$ are contained in the equations, an ascending series will converge, if x be less than the least root $\pm\alpha$, and $\pm(a-b)$ and $\pm(a+b)$; or more generally, if x be less than the least root $\pm\alpha$, and x^{n+1} at an infinite distance n, be infinitely less than

$$\frac{2a^n - 2 \cdot n \cdot \frac{n-1}{2} a^{n-2}b^2 + 2 \cdot n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} a^{n-4}b^4 - \&c.}{(a^2+b^2)^n} :$$

a descending series will always converge, when x is greater than the greatest root of the resulting equations; and x^{n-1}, when n is infinite, is infinitely greater than $(a+b)^x$ and $(a-b)^x$; or more generally than $2a^n - 2n \cdot \frac{n-1}{2} a^{n-2}b^2 + 2n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} a^{n-4}b^4 - \&c.$

This follows from Caput 3. of the Meditationes Algebraicæ.

Cor. It appears from hence, that, if the ascending series converges, the descending will diverge; and, *vice versâ*, if the descending converges, the ascending will diverge, unless all the roots of the above-mentioned resulting equations may be deemed

of

of equal magnitude, as $+\alpha$ and α, $\alpha\sqrt[n]{-1}$, &c. and $x=\alpha$; in which case sometimes both series may become the same converging series, &c.

When x, in the preceding cases, is equal to the least or greatest root, the series will sometimes converge, and sometimes not, as is shewn in the above-mentioned chapter. Whether the sum of a series, whose general term is given, can be found or not, will in many cases appear from the law of the multinomial and other more general series.

2. There are series which always converge, whatever may be the value of x; as, for example, the series $1+\frac{1}{2\cdot 3}x+\frac{1}{2\cdot 3\cdot 4\cdot 5}x^3+$ &c. or $1+\frac{x}{2^2}+\frac{x^2}{3^3}+\frac{x^3}{4^4}+$ &c. &c. always converge, whatever may be the value of x; but it may be observed, that these series never arise from the expansion of algebraical functions of x, or the before-mentioned fluents; but, in a few cases, they may from fluxional equations. There are also series which never converge as $1+1\cdot 2x+1\cdot 2\cdot 3x^2+1\cdot 2\cdot 3\cdot 4x^3+$ &c. to which the preceding remark may be applied.

3. In the year 1757 some papers, which contained the first edition of my Meditationes Algebraicæ, were sent to the Royal Society, in which was contained the following rule, *viz.* let S be a given function of the quantity x, which expand into a series $(a+bx^m+cx^{2m}+$ &c.) proceeding according to the dimensions of x; in the quantity S, for x^m write αx^m, βx^m, γx^m, &c. where α, β, γ, &c. are roots of the equation $x^n-1=0$; and let the resulting quantities be A, B, C, D, &c. then will $\frac{A+B+C+D+\text{&c.}}{n}$ be equal to the sum of the first, $2n+1$, $3n+1$, &c. terms *in infinitum*. This method, in the preface to the

laſt edition of the Meditationes Algebraicæ, is rendered more correct and general.

4. Let the ſum of a ſeries required be expreſſed by a function of a quantity z, the diſtance from the firſt term of the ſeries, then will the general term be the difference between the two ſucceſſive ſums generally expreſſed.

5. Let the general term be an algebraical function of z: 1ſt, let it be $\frac{az^m+bz^{m-1}+cz^{m-2}+\&c.}{z+e\,.\,z+e+1\,.\,z+e+2\,\ldots\,z+e+n-1}=T$, where m and n are whole numbers; and m (if the ſum of an infinite ſeries of terms is required) leſs than n by two or more: then the general term $\frac{az^m+bz^{m-1}+\&c.}{z+e\,.\,z+e+1\,.\,z+e+2\,\ldots\,z+e+n-1}=$

$$\frac{\gamma}{z+e\,.\,z+e+1}+\frac{\delta}{z+e\,.\,z+e+1\,.\,z+e+2}+$$

$$\frac{\varepsilon}{z+e\,.\,z+e+1\,.\,z+e+2\,.\,z+e+3}+\&c.\ldots\frac{\theta}{z+e\,.\,z+e+1\,\ldots\,z+e+n-1};$$

whence if

$z+e+2\,.\,z+e+3\,.\,z+e+4\,\ldots\,z+e+n-1=z^{n-3}+Az^{n-4}+Bz^{n-5}+\&c.$;

$z+e+3\,.\,z+e+4\,.\,z+e+5\,\ldots\,z+e+n-1=z^{n-3}+A'z^{n-4}+B'z^{n-5}+\&c.$;

$z+e+4\,.\,z+e+5\,\ldots\,z+e+n-1=z^{n-4}+A''z^{n-5}+B''z^{n-6}+\&c.$ and ſo on; then, if $m=n-2$, will $\gamma=a$, $\delta=b-\gamma A$, $\varepsilon=c-\delta A'-\gamma B$, $\zeta=d-\varepsilon A''-\delta B'-\gamma C$, &c.; whence the integral *in infinitum*, or ſum of the infinite ſeries, will be $\frac{\gamma}{z+e}+\frac{\delta}{2\,.\,z+e\,.\,z+e+1}+$

$$\frac{\varepsilon}{3\,.\,z+e\,.\,z+e+1\,.\,z+e+2}+\&c.$$

The reduction of the general term T into quantities of the before given formulæ was publiſhed in the Meditationes, printed in the year 1774. It was before reduced into formulæ of the ſame kind nearly by Mr. NICHOLE in the Paris Acts.

2d, Let the general term be $T'=$

$$\frac{az^b+bz^{b-1}+cz^{b-2}+\&c.}{z+e\,.\,z+e+1\,.\,z+e+2\ldots z+e+n-1\times z+f\,.\,z+f+1\ldots z+f+n-1\times z+g\,.\,z+g+1\ldots z+g+n-1\times\&c}$$

where

where h is a whole number leſs than $n+m+l+$&c. (if it be greater, then the fraction can eaſily be reduced into a rational quantity $az^{h-n-m-l-\&c.}+$&c. and a fraction of the before-mentioned kind); then will $T'=\left(\frac{\alpha}{z+e}+\frac{\alpha'}{z+f}+\frac{\alpha''}{z+g}+\&c.\right)+$

$$\left(\frac{\beta}{z+e\,.\,z+e+1}+\frac{\beta'}{z+f\,.\,z+f+1}+\frac{\beta''}{z+g\,.\,z+g+1}+\&c.\right)+$$

$$\left(\frac{\gamma}{z+e\,.\,z+e+1\,.\,z+e+2}+\frac{\gamma'}{z+f\,.\,z+f+1\,.\,z+f+2}+\frac{\gamma''}{z+g\,.\,z+g+1\,.\,z+g+2}\right.$$

$$\left.+\&c.\right)\ldots\left(\frac{X}{z+e\,.\,z+e+1\,.\,.\,z+e+n-1}+\frac{X'}{z+f\,.\,z+f+1\,.\,.\,z+f+m-1}\right.$$

$$\left.+\frac{X''}{z+g\,.\,z+g+1\,.\,.\,z+g+l-1}+\&c.\right)$$; whence its integral *in infinitum*, that is, the ſum of the infinite ſeries can be found when $\alpha=0$, $\alpha'=0$, $\alpha''=0$, &c.; and conſequently h not greater than $n+m+l+$&c. -2; otherwiſe not. If h is not greater than $n+m+l+$&c. -2, then will $\alpha+\alpha'+\alpha''+$&c. $=0$, for elſe the ſum would be infinite.

Let the number of quantities (e, f, g, &c.) be r, then from r independent integrals of a ſeries, whoſe term is T'; or from $(r-1)$ independent ſums of infinite ſerieſes, whoſe term is T'; that is, where h is not greater than $n+m+l+$&c. -2; can be deduced the ſum of all infinite ſerieſes of the before-mentioned formulæ, whoſe general term is T'.

If any factors are deficient in the denominator, as ſuppoſe the term to be $z+e\times z+e+3\times z+e+n-1$; multiply the numerator and denominator by the deficient factors, *viz.* by $z+e+1\,.\,z+e+2\times z+e+4\,.\,z+e+5\,.\,.\,z+e+n-2$, and it acquires the preceding formula; and ſo in the following examples.

3d, Let the denominator be $\overline{x+e}^{\pi}\times\overline{x+e+1}^{\pi}\times\overline{x+e+2}^{\pi}\ldots\overline{x+e+n-1}^{\pi}\times\overline{x+e+\mu}^{\pi'}\times\overline{x+e+\mu+1}^{\pi'}\times$

$x+e$

$\overline{x+e+\mu+2}^{\pi'} \times$ &c. $\times \overline{x+f}^{\varrho} \times \overline{x+f+1}^{\varrho} \times x+f+2^{\varrho} \ldots \times$ $\overline{x+f+m-1}^{\varrho} \times$ &c. $= D$, where π, π', ϱ, &c.; μ, &c. are whole numbers; and the general term is $\frac{ax^b+bx^{b-1}+cx^{b-2}+\&c.}{D} = T''$; then, if the dimensions of x in the numerator be less than its dimensions in the denominator, will $T'' =$

$$\left(\frac{\alpha}{x+e}+\frac{\alpha'}{(x+e)^2}+\frac{\alpha''}{(x+e)^3}\cdots\frac{\alpha^{\pi+\pi'-1}}{(x+e)^{\pi+\pi'}}+\frac{\beta}{x+f}+\frac{\beta'}{(x+f)^2}+\frac{\beta''}{(x+f)^3}\cdots\right.$$

$$\left.\frac{\beta^{\varrho-1}}{(x+f)^{\varrho}}+\&c.\right)+\left(\frac{\gamma}{x+e\,.\,x+e+1}+\frac{\theta}{x+f\,.\,x+f+1}+\&c.\right)+\&c.;$$

and in general there will be included all terms of the formulæ,

$$\frac{A\left(\overline{x+e+i}^{p}-\overline{x+e}^{p}\right)}{(x+e)^{p}\,.\,(x+e+1)^{p}\cdots(x+e+i)^{p}},$$

$$\frac{B\left((x+f+i')^{p'}-(x+f)^{p'}\right)}{(x+f)^{p'}\,.\,(x+f+1)^{p'}\cdots(x+f+i')^{p'}},\ \&c.$$

$$\frac{C\left((x+e+\mu+i'')^{p''}-(x+e+\mu)^{p''}\right)}{(x+e+\mu)^{p''}\,.\,(x+e+\mu+1)^{p''}\cdots(x+e+\mu+i'')^{p''}},\ \&c.$$

where A, B, C, &c. α, α', &c. β, β', &c. γ, θ, &c. denote invariable quantities; and p, p', p'', &c. are whole numbers not greater than π, ϱ, π', &c. respectively; and i, i', i'', &c. are whole numbers not greater than $n-1$, $m-1$, &c.

If all the quantities α, α', α'', &c. β, β', β'', &c. &c. are $=0$, the sum of the series can be expressed in finite terms of the quantity x, otherwise not; and also if b be less than the dimensions of x in the denominator by two or more, then will $\alpha+\beta+\&c.=0$, otherwise the sum would be infinite.

From $\pi+\pi'+\varrho+\&c.-1$ independent sums of infinite serieses of this kind can be deduced the sums of all infinite serieses of the same kind.

This method may be extended to infinite series, in which exponentials as e^x are contained, which will easily be seen from some subsequent propositions; but in my opinion the subsequent method of finding the sum of series is to be preferred to the preceding one, both for its generality and facility.

6. 1. Let the general term be $(az^b + bz^{b-1} + cz^{b-2} + \&c.) \times (z+e)^{-1} \cdot (z+e+1)^{-1} \cdot (z+e+2)^{-1} \ldots (z+e+n-1)^{-1}$; where b is a whole number less than n by two or more, when the sum of an infinite series is required.

Assume for the sum the quantity $(z+e)^{-1} \cdot (z+e+1)^{-1} \cdot (z+e+2)^{-1} \ldots (z+e+n-2)^{-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.)$; find the difference between this sum and its succeeding one $(z+e+1)^{-1} \cdot (z+e+2)^{-1} \cdot (z+e+3)^{-1} \ldots (z+e+n-1)^{-1} \times (\alpha \overline{z+1}^{b'} + \beta \overline{z+1}^{b'-1} + \&c.)$, which will be $-(z+e)^{-1} \cdot (z+e+1)^{-1} \cdot (z+e+2)^{-1} \ldots (z+e+n-1)^{-1} \times (\overline{z+e} \times (\alpha \overline{z+1}^{b'} + \beta \overline{z+1}^{b'-1} + \&c.) - \overline{z+e+n-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \&c.) = \overline{b'-n+1}\, \alpha z^{b'} + \&c.)$; then make the terms of this difference equal to the correspondent terms of the given quantity $az^b + bz^{b-1} + \&c.$ and there result $b' = b$, $-\overline{b-n+1} \times \alpha = a$, and consequently $\alpha = \frac{-a}{b-n+1}$, &c.

2. Let the general term be $(z+e)^{-1} \cdot (z+e+1)^{-1} \cdot (z+e+2)^{-1} \ldots (z+e+n-1)^{-1} \times (z+f)^{-1} \cdot (z+f+1)^{-1} \cdot (z+f+2)^{-1} \ldots (z+f+m-1)^{-1} \times (az^b + bz^{b-1} + cz^{b-2} + \&c.)$. Assume the quantity $(z+e)^{-1} \cdot (z+e+1)^{-1} \ldots (z+e+n-2)^{-1} \times (z+f)^{-1} \cdot (z+f+1)^{-1} \cdot (z+f+2)^{-1} \ldots (z+f+m-2)^{-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.)$ for the sum of the series sought; and thence deduce the general term, which suppose equal to the given general term, and from equating their corresponding parts easily can be deduced the index b' and co-efficients α, β, γ, &c. and consequently the sum of the series sought.

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3. Let the general term reduced to its loweſt dimenſions be $\overline{z+e}^{\pi} \times \overline{z+e+1}^{\rho} \ldots \overline{z+e+n-1}^{\sigma} \times \overline{rz+f}^{-\xi} \times \overline{rz+f+r}^{-\eta} \times \overline{rz+f+2r}^{-\theta} \ldots \overline{rz+f+m-1r}^{-\lambda} \times \overline{z+g}^{-\varepsilon} \times \overline{z+g+1}^{-\mu} \times \ldots \overline{z+g+l-1}^{-\nu} \times$ &c. $\times (az^{b} + bz^{b-1} + cz^{b-2} +$ &c.). If it be required to reduce the term $\overline{rz+f}^{-\xi}$, &c. to a conformity with the reſt, for $\overline{rz+f}^{-\xi}$, &c. ſubſtitute $\overline{z+\frac{f}{r}}^{-\xi} \times r^{-\xi}$, &c. and it is done. Aſſume for the integral or ſum the quantity $S = \overline{z+e}^{\pi} . \overline{z+e+1}^{\rho} \ldots \overline{z+e+n-2}^{\sigma} \times \overline{rz+f}^{-\xi} . \overline{rz+r+f}^{-\eta} \ldots \overline{rz+m-2r+f}^{-\theta} \times \overline{z+g}^{-\varepsilon} \times \overline{z+g+1}^{-\mu} \ldots \overline{z+g+l-2}^{-\nu} \times$ &c. $\times (\alpha z^{b'} + \beta z^{b'-1} +$ &c.) $= S$, find its ſucceſſive ſum by writing $z+1$ for z in the ſum S, and let the quantity reſulting be S′; then will the general term be $S - S'$, which equate to the given general term, that is, their correſpondent quantities; and thence may be deduced the index b' and co-efficients α, β, &c.; and conſequently the ſum ſought. If the ſeries does not terminate, then the ſum will be expreſſed by a ſeries proceeding *in infinitum*, according to the reciprocal dimenſions of z.

From $\pi + \rho + \sigma +$ &c. $- 1$ independent integrals of the above-mentioned kind can be deduced the integrals of all quantities of the ſame kind; that is, where b is any whole affirmative number whatever, and the co-efficients a, b, c, &c. are any how varied.

If any factor $z+g$ in the denominator, &c. has no other $z+g+l-1$, which differs from it by a whole number $l-1$; or the factor $rz+f$ has no correſpondent factor $rz+f+mr$, where m is a whole number; then the integral of the above-mentioned ſeries cannot be expreſſed in finite terms of the quantity z. In like manner, if the dimenſions of z in the numerator are leſs

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than its dimenſions in the denominator by unity, then the integral of the general term cannot be expreſſed by a finite algebraical function of z. If the number of terms to be added be infinite, it is well known that the ſum in this caſe will be infinite.

It may be obſerved, that in finding the ſum of a ſeries, whoſe general term is given, all common diviſors of the numerator and denominator muſt be rejected, otherwiſe ſerieſes may appear difficult to be ſummed, which are very eaſy: for example, let the ſeries be $\frac{1}{1.2.3.4.5}+\frac{4}{4.5.6.7.8}+\frac{9}{7.8.9.10.11}+\&c.=\frac{1}{3}\left(\frac{1}{1.2.4.5}+\frac{2}{4.5.7.8}+\frac{3}{7.8.10.11}+\&c.\right)$, whoſe general term is $\frac{z+1}{3z+1.3z+4\times 3z+2.3z+5}$; and by aſſuming, as s before taught, $\overline{3z+1}^{-1}\times\overline{3z+2}^{-1}\times a$ for the ſum ſought; and finding its general term $\overline{3z+1}^{-1}\times\overline{3z+4}^{-1}\times\overline{3z+2}^{-1}\times\overline{3z+5}^{-1}\times 18\overline{z+1}\times a$, which equating to the general term given, there reſults $18a=1$, and the ſum ſought $=\frac{1}{18}\times\frac{1}{3z+1.3z+2}$.

Ex. 2. Let the ſeries be $\frac{1}{1.2.3.4.5}+\frac{14}{5.6.7.8.9}+\frac{55}{9.10.11.12.13}+\frac{140}{13.14.15.16.17}+\&c.=\frac{1}{24}\left(\frac{1}{1.5}+\frac{1}{5.9}+\frac{1}{9.13}+\frac{1}{13.17}+\&c.\right)$, of which the general term is $\frac{1}{24}\times\frac{1}{4z+1.4z+5}$; and conſequently the ſum deduced is $\frac{1}{24}\times\frac{1}{4}\times\frac{1}{4z+1}$.

Theſe are ſerieſes given by Mr. De Moivre, and eſteemed by Dr. Taylor *altioris indaginis*.

Some other writers have made ſome ſerieſes to appear more difficult to be ſummed, by not reducing them to their loweſt terms.

7. Having given the principles of a general method of finding the ſum of a ſeries, when its general term can be expreſſed by algebraical, and not exponential, functions of z, the diſtance from the firſt term of the ſeries; it remains to perform the ſame when exponentials are included.

1. Let S the ſum be any algebraical function of z multiplied into $e^z = x^z$; then will the general term be $Se^z - eS'e^z = (S - eS')e^z$; whence, from the general term Te^z being given, aſſume quantities in the ſame manner (with the ſame denominator, &c.) as when no exponential was involved, which multiplied into e^z, ſuppoſe to be the ſum; from the ſum find its general term, and equate it to the given one by equating their correſpondent co-efficients, and it is done.

Ex. Let the general term be $\frac{z+2}{2z+1 \cdot 2z+3} \times e^{z+1}$: aſſume for the ſum ſought $\frac{\alpha}{2z+1} \times e^{z+1}$, whence the general term is $\left(\frac{\alpha}{2z+1} - \frac{\alpha e}{2z+3}\right) e^{z+1} = \frac{2\alpha(1-e)z + 3\alpha - \alpha e}{2z+1 \cdot 2z+3} \times e^{z+1}$; equate it to the given term, and there reſults $2\alpha(1-e) = 1$ and $3\alpha - \alpha e = 2$, and conſequently $e = \frac{1}{4}$ and $\alpha = \frac{4}{3}$, if the ſeries can be ſummed.

The ſame obſervation, *viz.* that if any factor in the denominator or irrational quantity have no other correſpondent to it; for example, if the factor be $z+g$, and there is no correſpondent one $x+g+n$, where n is a whole number, then its integral cannot be expreſſed by a finite algebraical function of z.

In the ſame manner may the ſums be found, when the terms are exponentials of ſuperior orders; for the exponential, irra-

tional, &c. quantities in the denominators of the ſums may be eaſily deduced from the preceding principles; and thence, by proceeding as is before taught, the ſum required.

The principles of all theſe caſes have been given in the Meditationes.

8. Mr. JAMES BERNOULLI found ſummable ſerieſes by aſſuming a ſeries V, whoſe terms at an infinite diſtance are infinitely little, and ſubtracting the ſeries diminiſhed by any number (i) of terms from the ſeries itſelf, &c.

It is obſerved in the Meditationes; that if $T(m)$, $T(m+n)$, $T(m+n+n')$, $T(m+n+n'+n'')$, &c. be the terms at m, $m+n$, $m+n+n'$, $m+n+n'+n''$, &c. diſtances from the firſt, and $aT(m)+bT(m+n)+cT(m+n+n')+dT(m+n+n'+n'')+$&c. be the general term, it will be ſummable, when $a+b+c+d+$&c. $=0$; the ſum of the ſeries will be $a(T(m)+T(m+1)+T(m+2)+\ldots+T(m+n+n'+n''+$&c.$-1))+b(T(m+n)+T(m+n+1)+T(m+n+2)+\ldots+T(m+n+n'+n''+$&c.$-1))+c(T(m+n+n')+T(m+n+n'+1)+\ldots(T(m+n+n'+n''+$&c.$-1))+$&c. $=H$. If the ſum $a+b+c+d+$&c. be not $=0$, and the ſeries $T(m)+T(m+1)+T(m+2)+$&c. *in infinitum* be a converging one $=S$, then will the ſum of the reſulting ſeries be $(a+b+c+d+$&c.$)S-(b+c+d+$&c.$)(T^{m}\ldots+T^{m+n-1})-(c+d+$&c.$)(T^{m+n}\ldots T^{m+n+n'-1})-(d+$&c.$)(T^{m+n+n'}+\ldots T^{m+n+n'+n''-1})-$&c.

8. 2. Let the ſeries V conſiſt of terms, which have only one factor in the denominator, and its numerator $=1$; that is, let the general term be $\frac{1}{rz+e}$, and the ſeries conſequently $\frac{1}{e}+\frac{1}{r+e}+\frac{1}{2r+e}+$&c. $=V$; from the before-mentioned addition or ſubtraction there follows $\frac{a}{rz+e}+\frac{b}{rz+r+e}+\frac{c}{rz+2r+e}+$&c. $=$

$\frac{\alpha z^m+\beta z^{m-1}+\gamma z^{m-2}+\&c.}{rz+e\,.\,rz+r+e\,.\,rz+2r+e\,.\,\&c.}$; where m is not greater than the number (N) of factors in the denominator diminished by unity. From α, β, γ, &c. r and e being given, easily can be acquired by simple equations, or known theorems, the required co-efficients a, b, c, &c. If $m=N-1$ and α and $a+b+c+d+\&c.=0$, then the sum of the series resulting will be finite.

8. 3. If the terms of the series assumed $\frac{1}{e}-\frac{1}{r+e}+\frac{1}{2r+e}-\frac{1}{3r+e}$ + &c. be alternately affirmative and negative; then by the preceding case find $\frac{\alpha z^m+\beta z^{m-1}+\gamma z^{m-2}+\&c.}{rz+e\,.\,rz+r+e\,.\,rz+2r+e+\&c.}=$ $\frac{a}{rz+e}+\frac{b}{rz+r+e}+\frac{c}{rz+2r+e}+\&c.$ Where the terms of the resulting series are alternately affirmative and negative, let the two subsequent terms be supposed $\frac{\alpha z^m+\beta z^{m-1}+\gamma z^{m-2}+\&c.}{rz+e\,.\,rz+r+e\,\ldots\,rz+n-1r+e}$ $=\frac{a}{rz+e}+\frac{b}{rz+r+e}+\&c.$ and $\frac{\alpha z+1^m+\beta z+1^{m-1}+\gamma z+1^{m-2}+\&c.}{rz+r+e\,.\,rz+2r+e\,\ldots\,rz+nr+e}=$ $\frac{a}{rz+r+e}+\frac{b}{rz+2r+e}+\&c.$ of which the one is affirmative and the other negative: reduce the resulting series to an affirmative one by subtracting the subsequent term from its preceding, and it becomes $\frac{(rz+nr+e)\,\overline{(\alpha z^m+\beta z^{m-1}+\&c.)}-(rz+e)\,\overline{(\alpha z+1^m+\beta z+1^{m-1}+\&c.)}}{rz+e\,.\,rz+r+e\,.\,rz+2r+e\,\ldots\,rz+rn+e}$ $=\frac{\overline{n-mr}\alpha z^m+\&c.}{rz+e\,.\,rz+r+e\,\ldots\,rz+rn+e}=\frac{a}{rz+e}+\frac{b-a}{rz+r+e}+\&c.$ In this case, since two terms are added into one, the distance from the first term of the series will be $\frac{z}{2}$, which suppose $=w$; and write $2w$ for z in the above-mentioned term, and there results $\frac{\overline{n-mr}\alpha z^m+\&c.}{rz+e\,.\,rz+r+e\,\ldots\,rz+nr+e}=\frac{\overline{n-mr}\alpha\times 2^m w^m+\&c.}{2rw+e\,.\,2rw+r+e\,\ldots\,2rw+nr+e}=$ $=\frac{a}{2rw+e}+$

$\frac{a}{2rw+e}+\frac{b-a}{2rw+r+e}+$ &c.; whence the sum of any series, whose general term is $\frac{a'w^m+b'w^{m-r}+\text{&c.}}{2rw+e.2rw+r+e\ldots 2rw+m+e}$, where m is a whole number less than n by two or more, and w the distance from the first term of the series can be found from the sum of the series $\frac{1}{e}-\frac{1}{r+e}+\frac{1}{2r+e}-\frac{1}{3r+e}+$ &c.

9. Let there be two series $\frac{1}{e}+\frac{1}{e+r}+\frac{1}{e+2r}+$ &c. $=S$ and $\frac{1}{f}+\frac{1}{f+r}+\frac{1}{f+2r}+\frac{1}{f+3r}+$ &c. $=S'$, whose general terms are respectively $+\frac{1}{e+rz}$ and $+\frac{1}{f+rz}$; then from the sum of these two series can be collected the sum of any series, whose general term is

$$\frac{az^m+\beta z^{m-1}+\text{&c.}}{rz+e.rz+e+r.rz+e+2r\ldots rz+n-1r+e\times rz+f.rz+r+f\ldots rz+f+m-1r}$$

$$=\frac{a}{rz+e}+\frac{b}{rz+e+r}+\frac{c}{rz+e+2r}\cdots+\frac{\lambda}{rz+n-1r+e}+\frac{a'}{rz+f}+\frac{b'}{rz+r+f}$$

$$+\frac{c'}{rz+2r+f}\cdots+\frac{\mu'}{rz+m-1r+f}$$; where $e-f$ is not a whole number. Let $a+b+c\ldots+\lambda=0$, and $a'+b'+c'\ldots+\mu'=0$, then the sum will be $a\left(\frac{1}{rz+e}+\frac{1}{rz+r+e}\cdots+\frac{1}{rz+n-2r+e}\right)+b\left(\frac{1}{rz+e+r}\right.$

$$\left.+\frac{1}{rz+e+2r}\cdots+\frac{1}{rz+n-2r+e}\right)+c\left(\frac{1}{rz+e+2r}+\frac{1}{rz+e+3r}+\ldots+\right.$$

$$\left.\frac{1}{rz+n-2r+e}\right)+\text{&c.}+a'\left(\frac{1}{rz+f}+\frac{1}{rz+r+f}\cdots\frac{1}{rz+m-2r+f}\right)+b'$$

$$\left(\frac{1}{rz+r+f}+\ldots+\frac{1}{rz+m-2r+f}\right)+\text{&c.}$$

2. If the series are $\frac{1}{e}-\frac{1}{e+r}+\frac{1}{e+2r}-$ &c. and $\frac{1}{f}-\frac{1}{f+r}+\frac{1}{f+2r}$ $-$ &c.; then from the sum of these two series can be collected by the principles given above the sum of any series, whose general term is

$$\frac{az^m+\beta z^{m-r}+\gamma z^{m-2r}+\&c.}{2rz+r.2rz+r+s.2rz+2r+s\ldots2rz+m-1r+s\times2rz+f.2rz+r+f.2rz+2r+f\ldots2rz+m-1r+f}.$$

The same principle may be applied to find the sum of any series of the abovementioned sort, in whose denominator are contained other factors, $rz+g$, $rz+g+r$, &c. &c.; or $2rz+g$, $2rz+g+r$, $2rz+g+2r$, &c. Like propositions may be deduced from series's, in which r and r', &c. and the factors $rz+e$ and $r'z+g$, &c. denote different quantities.

10. An apparently more general method may be given from assuming a series or series's as before; and adding every two, three, four, &c. (n) successive terms together for terms of a new series beginning from the first, second, third, &c. n^{th} term; and in general adding together two, three, &c. n successive general terms; and in their sum writing for z the distance from the first term of the series $az+a$, $3z+a$, &c. $nz+a$; there will result the general term of a series not to be found from the above-mentioned addition.

Ex. Let the series assumed be $\frac{1}{1}+\frac{1}{2}+\frac{1}{3}+\frac{1}{4}+$ &c. *in infinitum*, of which the general term beginning from the first is $\frac{1}{z+1}$; add three successive general terms $\frac{1}{z+1}+\frac{1}{z+2}+\frac{1}{z+3}=\frac{3z^2+12z+11}{z+1.z+2.z+3}$; in this term for z write $3z$, and there results $\frac{27z^3+36z+11}{3z+1.3z+2.3z+3}$. In the same manner, if the beginning is instituted from the second or third term of the given series, the terms resulting will be $\frac{3z^2+18z+26}{z+2.z+3.z+4}$ and $\frac{3z^2+24z+47}{z+3.z+4.z+5}$. In these terms for z write $3z$, and there result $\frac{27z^3+54z+26}{3z+2.3z+3.3z+4}$ and $\frac{27z^3+72z+47}{3z+3.3z+4.3z+5}$.

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If the terms of the given series are alternately affirmative and negative, the terms of the resulting series will be alternately affirmative and negative, if n be an odd number; otherwise its terms will be all affirmative. The sum of this series will be finite or infinite, as the sum of the series $1+\frac{1}{2}+\frac{1}{3}+\frac{1}{4}+$ &c. is finite or infinite; but from it, by the preceding method of addition or subtraction of Mr. BERNOULLI's, or a like method applied to more series, may be found the sums of different finite series.

It may be observed, that from Mr. BERNOULLI's addition or subtraction can never be deduced the series which arise from this method; for, by his method, the denominator can never have any factors but what are contained in the denominators of the given series, *viz.* (in the series $\frac{1}{1}+\frac{1}{2}+\frac{1}{3}+$ &c.), $z+l$, where l is a whole number; but by this method are introduced into the denominator the factors $2z+l$, $3z+l$, &c. and $nz+l$, or which may be reduced to the same $\left(z+\frac{l}{n}\right)\times n$.

If n successive general terms of the series arising from Mr. BERNOULLI's addition or subtraction be added together, and in the quantity thence arising for z the distance from the first term of the series be substituted nz, there will be produced series of the above-mentioned formula.

11. Multiply two converging series $a+bx+cx^2+dx^3+$ &c. $=S$ and $\alpha+\beta x+\gamma x^2+$ &c. $=V$, or find any rational and integral function of them, and the series resulting will be finite and $=S\times V$, &c. Let $\alpha+\beta x+\gamma x^2+$ &c. $x^m=V$ be finite, and the resulting series will be finite and $=S\times V$, &c. If S be a series converging or not, whose ultimate terms are less than any finite quantity, then will the series $(a+bx+cx^2+$ &c.$)\times(\alpha+\beta x+\gamma x^2+$ &c. $x^m)=V\times S$ be a converging one, if $\alpha+\beta x+\gamma x^2+\ldots$ &c. $x^m=0$; which case was given by Mr. DE MOIVRE.

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Mr. BERNOULLI's addition, &c. can be applied to ſeries of this kind. For example, let the given ſeries be $\frac{1}{e}+\frac{1}{e+1}x+\frac{1}{e+2}x^2+$ &c. $=$ S. From this ſeries ſubtract the ſame ſeries diminiſhed by m terms, *viz.* $\frac{1}{e+m}x^m+\frac{1}{e+m+1}x^{m+1}+\frac{1}{e+m+2}x^{m+2}+$ &c. and there remains $\frac{e+m-ex^m}{e\,.\,m+e}+\frac{e+m+1-\overline{e+1}x^m}{e+1\,.\,e+m+1}x+\frac{e+m+2-\overline{e+2}x^m}{e+2\,.\,e+m+2}x^2+\frac{e+m+3-\overline{e+3}x^m}{e+3\,.\,e+m+3}\times x^3+$ &c.; for x^m write A, then will the ſeries become $\frac{m-eA}{e\,.\,m+e}+\frac{e+m+1-\overline{e+1}A}{e+1\,.\,e+m+1}x+\frac{e+m+2-\overline{e+2}A}{e+2\,.\,e+m+2}x^2+\frac{e+m+3-\overline{e+3}A}{e+3\,.\,e+m+3}x^3+$ &c. $=\frac{1}{e}+\frac{1}{e+1}x+\frac{1}{e+2}x^2\ldots\ldots\frac{1}{e+m-1}x^{m-1}$.

Let the general term be $\frac{ax^m+bx^{m-1}+cx^{m-2}+\text{&c.}}{z+e\,.\,z+e+1\,.\,z+e+2\,.\,.\,z+e+n-1}\times x^z$ $=\left(\frac{\alpha}{z+e}+\frac{\beta}{z+e+1}+\frac{\gamma}{z+e+2}\ldots\frac{X}{z+e+n-1}\right)x^z$. Suppoſe $\beta=\beta' x$, $\gamma=\gamma' x^2$, $\delta=\delta' x^3$, .. $X=X' x^{n-1}$; then will the ſum of the above-mentioned ſeries be $(\alpha+\beta'+\gamma'+\delta'+$ &c.$)\times S-\frac{1}{e}$ $(\beta'+\gamma'+\delta'+$ &c.$)-\frac{1}{e+1}(\gamma'+\delta'+$ &c.$)-\frac{1}{e+2}(\delta'+$ &c.$)-$ &c.

From the ſum of the ſeries $\frac{1}{e}-\frac{x}{e+1}+\frac{x^2}{e+2}-$ &c. by theſe and the principles before delivered can be deduced the ſum of any ſeries, whoſe general term is

$$\frac{ax^m+bx^{m-1}+\text{&c.}}{2z+e\,.\,2z+e+1\,.\,2z+e+2\,.\,2z+e+3\times\text{&c.}}x^m.$$

In like manner from the ſum of the ſeries $\frac{x}{e}+\frac{x^2}{e+1}+\frac{x^3}{e+2}+$ &c. $\frac{x}{f}+\frac{x^2}{f+1}+\frac{x^3}{f+2}+$ &c. $\frac{x}{g}+\frac{x^2}{g+1}+\frac{x^3}{g+2}+$ &c. &c. can be deduced

deduced the sum of any series, whose general term is

$$\frac{az^m+bz^{m-1}+\&c.}{z+e.z+e+1.z+e+2\&c.\times z+f.z+f+1.z+f+2.\&c.\times z+g.z+g+1.\&c.}\times x^z.$$

And also from the sum of the series $\frac{1}{e}-\frac{1}{e+1}x+\frac{x^2}{e+2}-$ &c. $\frac{1}{f}-\frac{x}{f+1}+\frac{x^2}{f+2}-$&c. $\frac{1}{g}-\frac{x}{g+1}+\frac{x^2}{g+2}-$&c. &c. can be deduced the sum of any series, whose general term is

$$\frac{az^m+bz^{m-1}+\&c.}{2z+e.2z+e+1.\&c.\times 2z+f.2z+f+1.\&c.2z+g.2z+g+1.\&c.}\times x^z.$$

The method of adding more terms of a given series together, as before taught, may be applied to these and all other series. For example: let the given series be $1+\frac{1}{2}x+\frac{1}{3}x^2+\frac{1}{4}x^3+$&c.; add two terms constantly together, and it becomes $1+\frac{1}{2}x+$&c. $=\frac{2+x}{2}+\frac{4+3x}{3.4}x^2+\frac{6+5x}{5.6}x^4+$&c. $=\frac{2+A}{2}+\frac{4+3A}{3.4}x^2+\frac{6+5A}{5.6}x^4+$&c. whence the general term is $\frac{2z+2+(2z+1)}{2z+2}\frac{x}{2z+1}x^{2z}$. From the methods before given of addition, subtraction, and multiplication; and the series found by this method, can be derived series, whose sums are known.

12. Suppose a given series $ax^n+bx^{n\pm r}+cx^{n\pm 2r}+dx^{n\pm 3r}+$&c. whose sum p is either an algebraical, exponential, or fluential fluxion of x; multiply the equation $p=ax^n+bx^{n\pm r}+cx^{n\pm 2r}+dx^{n\pm 3r}+$&c. into $x^{\pm r-s}$, and there results $ax^{\pm r-s}p=ax^{\pm r}+bx^{\pm r\pm r}+cx^{\pm r\pm 2r}+$&c.; find the fluxion of this equation, and there follows $\frac{1}{\dot{x}}$ multiplied into the fluxion of the quantity $(x^{\pm r-s}p)=\pm rax^{\pm r-1}+(\pm r\pm s)bx^{\pm r\pm r-1}+(\pm r\pm 2s)cx^{\pm r\pm 2r-1}+$&c. of which the general term is $(\pm r\pm zs)\times t$, where z denotes the distance from the first term of the series, and t is

is the term in the given ſeries, whoſe diſtance from the firſt is z. In the ſame manner may be deduced the ſum of a ſeries whoſe general term is $t' \times \pm r \pm zs \times \pm r' \pm z \pm ns$, or by repeated operations $t' \times ez^2 + fz + g$, where t' is a term of the given equation, whoſe diſtance from the firſt term is z. And in general, from the ſum of a given ſeries, whoſe fluxion can be found, and whoſe general term is t', can be deduced by continued multiplication, and finding the fluxion, the ſum of a ſeries or quantity, of which the general term is At', where A is any function of the following kind $a'z^m + b'z^{m-1} + c'z^{m-2}$ &c. in which z denotes the diſtance from the firſt term of the ſeries, and m a whole number. It is to be obſerved, that if the given ſeries converges in a ratio, which is at leaſt equal to the ratio of the convergency of ſome geometrical ſeries, the reſulting equation will always converge. But if in a leſs ratio, then it will ſometimes converge, ſometimes not, according to the ratio which the ſucceſſive terms of the reſulting ſeries have to each other at an infinite diſtance.

Corollary. $\frac{p \cdot p+1 \cdot p+2 \cdot p+3 \ldots p+z}{r \cdot r+1 \cdot r+2 \cdot r+3 \ldots r+z} =$ $\frac{p+z \cdot p+z-1 \cdot p+z-2 \cdot p+z-3 \ldots z+r+1}{r \cdot r+1 \cdot r+2 \cdot r+3 \ldots \ldots p-1}$, if $p-r$ be a whole affirmative number; but this latter quantity has the formula above-mentioned $az^m + bz^{m-1} + cz^{m-2}$ &c.; and conſequently, if the ſum of the ſeries $a + bx^1 + cx^{2.1} + dx^{3.1}$ &c. $= p$ be known, by this method can be deduced the ſum of the ſeries $a + \frac{p}{r} bx^1 + \frac{p \cdot p+1}{r \cdot r+1} cx^{2.1} + \frac{p \cdot p+1 \cdot p+2}{r \cdot r+1 \cdot r+2} dx^{3.1}$ &c.

Ex. 1. Since $\overline{a+x}^{\frac{m}{n}} = a^{\frac{m}{n}} \left(1 + \frac{m}{n} \times \frac{x}{a} + \frac{m}{n} \times \frac{m-n}{2n} a^{-2} x^2 + \frac{m}{n} \cdot \frac{m-n}{2n} \cdot \frac{m-2n}{3n} a^{-3} x^3 + \&c.\right)$; multiply the ſucceſſive terms of this ſeries into

into the terms of the series $1, \frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}$, &c. and a series is deduced $a^{\frac{m}{n}} + \frac{p \cdot m}{r \cdot n} a^{\frac{m}{n}-1} x + \frac{p \cdot p+1 \times m \cdot m-n}{r \cdot r+1 \cdot n \cdot 2n} x^{\frac{m}{n}-2}$ + &c. whose sum is known, if the sum of the series $= \overline{a+x}^{\frac{m}{n}}$ is known.

Ex. 2. If the series begins from the $l+1^{\text{th}}$ term of the above-mentioned binomial theorem $a^{\frac{m}{n}} + \frac{m}{n} a^{\frac{m}{n}-1} x$ + &c. *viz.* the series be $H \times 1 + \frac{m-l+1n}{l+2n} \frac{x}{a} + \frac{m-l+2n}{l+3n} \frac{x^2}{a^2} + \frac{m-l+3n}{l+4n} \frac{x^3}{a^3}$ + &c. of which let the respective terms be multiplied into $1, \frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}$, &c. there will result a series whose sum is known.

Ex. 3. From the rule first given by me for finding the sum of the terms at b distances from each other, the sum of the series $1 + \frac{m-l+1n}{l+2 \cdot n} \times \frac{m-l+2n}{l+3\ n} \ldots \frac{m-l+bn}{l+b+1n} \times \frac{x^b}{a^b} + P \times \frac{m-l+b+1n}{l+b+2n} \times \frac{m-l+b+2n}{l+b+3n} \ldots \frac{m-l+2bn}{l+2b+1n} \frac{x^{2b}}{a^{2b}}$ + &c. where P denotes the co-efficient of the preceding term, can be deduced; and consequently the sum of the series deduced from multiplying the successive terms of this series into the quantities $1, \frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}$, &c. respectively.

The general principles of this case were first delivered by Mr. Bernoulli, Mr. De Moivre, Mr. Euler, &c.

12. Assume the series $a + bx^n + cx^{2n}$ + &c. $= p$, multiply it into $x^{m-1}\dot{x}$, and find the fluent, then will $\frac{1}{m} x^m p - \frac{1}{m} \int x^m \dot{p} = \frac{1}{m} a x^m +$

$\frac{1}{\alpha+n} bx^{\alpha+n} + \frac{1}{\alpha+2n} cx^{\alpha+2n}$ + &c.; multiply this equation into $x^{\beta-\alpha-1}\dot{x}$, and find the fluent of the equation resulting, which will be $\frac{1}{\beta} \times \frac{1}{\alpha} x^{\beta}\dot{p} - \frac{1}{\alpha} \cdot \frac{1}{\beta} \int x^{\beta}\dot{p} - \frac{1}{\alpha} \times \frac{1}{\beta-\alpha} x^{\beta-\alpha} \int x^{\alpha}\dot{p} + \frac{1}{\alpha} \cdot \frac{1}{\beta-\alpha}$ $\int x^{\beta}\dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} ax^{\beta} + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} bx^{\beta+n} + \frac{1}{\alpha+2n} \cdot \frac{1}{\beta+2n} cx^{\beta+2n}$ + &c.; divide by x^{β}, and there results $\frac{1}{\beta} \cdot \frac{1}{\alpha} \dot{p} + \frac{1}{\alpha} \cdot \frac{1}{\alpha-\beta} x^{-\alpha} \int x^{\alpha}\dot{p} +$ $\frac{1}{\beta} \cdot \frac{1}{\beta-\alpha} x^{-\beta} \int x^{\beta}\dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} a + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} bx^{n}$ + &c.; and in general $\frac{1}{\alpha} \cdot \frac{1}{\beta} \cdot \frac{1}{\gamma}$. &c. $\dot{p} + \frac{1}{\alpha} \cdot \frac{1}{\alpha-\beta} \cdot \frac{1}{\alpha-\gamma} x^{-\alpha} \int x^{\alpha}\dot{p} + \frac{1}{\beta} \cdot \frac{1}{\beta-\alpha} \cdot \frac{1}{\beta-\gamma} \cdot$ &c. $x^{-\beta} \int x^{\beta}\dot{p} + \frac{1}{\gamma} \cdot \frac{1}{\gamma-\alpha} \cdot \frac{1}{\gamma-\beta}$, &c. $x^{-\gamma} \int x^{\gamma}\dot{p} = \frac{1}{\alpha} \cdot \frac{1}{\beta} \cdot \frac{1}{\gamma}$ &c. $a + \frac{1}{\alpha+n} \cdot \frac{1}{\beta+n} \cdot \frac{1}{\gamma+n}$ &c. $bx^{n} + \frac{1}{\alpha+2n} \cdot \frac{1}{\beta+2n} \cdot \frac{1}{\gamma+2n}$, &c. cx^{2n} + &c. whence the law of continuation is immediately manifeſt.

Hence, if no two quantities α, β, γ, δ, &c. be equal to each other; and the ſucceſſive terms a, b, c, d, &c. of any ſeries $a + bx^{n} + cx^{2n}$ + &c. $= p$ be divided by $\alpha . \beta . \gamma . \delta$. &c.; $\overline{\alpha+n} . \overline{\beta+n} .$ $\overline{\gamma+n} . \overline{\delta+n}$. &c.; $\alpha+2n . \beta+2n . \gamma+2n . \delta+2n$. &c. &c.; and in general by $\alpha+nz . \beta+nz . \gamma+nz . \delta+nz$. &c. &c.; then can the ſum of the ſeries be found from the fluents of the fluxions $x^{\alpha}\dot{p}$, $x^{\beta}\dot{p}$, $x^{\gamma}\dot{p}$, $x^{\delta}\dot{p}$, &c. as has been obſerved in the Meditationes. If two are equal, *viz.* $\alpha = \beta$, then alſo the fluent of the fluxion $\frac{\dot{x}}{x} \int x^{\alpha}\dot{p}$ is required. If three are equal *viz.* $\alpha = \beta = \gamma$; then it is neceſſary to find the fluent of the fluxion $\frac{\dot{x}}{x} \int \frac{\dot{x}}{x} \int x^{\alpha}\dot{p}$; and ſo on.

1. Let $\dot{p} = \frac{\dot{x}}{1 \pm x^{n}}$; and if the differences of the quantities α, β, γ, δ, &c. are diviſible by n, from the fluent of the fluxion

fluxion $x^{\alpha}\dot{p}$ can be deduced the fluents of all the other fluxions $x^{\beta}\dot{p}$, $x^{\gamma}\dot{p}$, &c.; and in general, if $\alpha - \beta$ is divisible by n, then from the fluent of the fluxion $x^{\alpha}\dot{p}$ can be deduced the fluent of the fluxion $x^{\beta}\dot{p}$.

2. Suppose p = the terms of the binomial theorem expanded according to the dimensions of x, *viz.* $(a+bx^n)^{\frac{r}{s}} = a^{\frac{r}{s}} + \frac{r}{s}a^{\frac{r}{s}-1}bx^n$ + &c. beginning from the first or any other terms; then, if α, β, &c. divided by n give whole affirmative numbers, will all the fluxions $x^{\alpha}\dot{p}$, $x^{\beta}\dot{p}$, $x^{\gamma}\dot{p}$, &c. be integrable; and if the differences of the quantities α, β, γ, δ, &c. are divisible by n, from the fluent of the fluxion $x^{\alpha}\dot{p}$ can be deduced the fluents of the fluxions $x^{\beta}\dot{p}$, $x^{\gamma}\dot{p}$, &c.

If $\dot{p}$ denotes the sum of the alternate or terms whose distance from each other are m, of the binomial theorem, the same may be applied.

3. If $p = \overline{a+bx^n+cx^{2n}}^{\frac{r}{s}}$; and α, β, γ, δ, &c. divided by n give whole affirmative numbers, then from $\int x^{\alpha}\dot{p}$ can be deduced all the remainder $\int x^{\beta}\dot{p}$, $\int x^{\gamma}\dot{p}$, &c.: and in general from two can be deduced all the remainder.

To find when the sum of any series of this kind can be found, add together each of the fluents, which can be found from each other, and not otherwise, and suppose their sum = o; and so of any other similar fluent, and from the resulting equations can be discovered when the series can be integrated.

13. If the general term of a series contains in it more variable quantities, z, v, w, &c.; then find the sum of the series, first, from the hypothesis that one of them (z) is only variable,

ble, which, properly corrected, let be A; in the quantity A ſuppoſe all the quantities invariable but ſome other v, and find the ſum of the ſeries thence reſulting, which let be B, and ſo on; and the ſum of the ſeries will be deduced.

Ex. Let the term be $\frac{1}{z.z+1\times v.v+1.v+2}$; the dimenſions of z and v, &c. in the denominator muſt be at leaſt greater than its dimenſions in the numerator by a quantity greater than the number of the quantities z, v, &c. which proceed *in infinitum* increaſed by unity. Firſt, ſuppoſe z only variable, and the ſum of the infinite ſeries reſulting will be $\frac{1}{z.v.v+1.v+2} = A$; then ſuppoſe v only variable, and the ſum reſulting will be $\frac{1}{2z.v.v+1} = B$, which is the ſum required.

If it be ſuppoſed, that the quantities z and v, &c. in the ſame term ſhall never have the ſame values, then ſuppoſe z and v always to have the ſame values, and the general term $\frac{1}{z.z+1.v.v+1.v+2}$ becomes $\frac{1}{z^2.z+1^2.z+2}$, of which let the ſum be V, then will B − V be the ſum required.

On this and ſome other ſubjects more have been given in the Meditationes.

14. If the ſum of the ſeries cannot be found in finite terms, and it is neceſſary to recur to infinite ſeries; it is obſerved in the Meditationes to be generally neceſſary to add ſo many terms together, that the diſtance from the firſt term of the ſeries may conſiderably exceed the greateſt root of an equation reſulting from the general term made = 0; and afterwards a ſeries more converging may commonly be deduced from the fluents of fluxions reſulting from neglecting all but the greateſt quantities in the general terms reſulting; and by other

different methods. Mr. NICHOLAS BERNOULLI and Mr. MOIVRE inveſtigated the ſum of the ſeries (P) $A+Br+Cr^2+$ &c. by a ſeries (Q) $\frac{A}{1-r}+\frac{d'r}{(1-r)^2}+\frac{d''r^2}{(1-r)^3}+\frac{d'''r^3}{(1-r)^4}+$ &c.; where d', d'', d''', &c. denote the ſucceſſive differences of the terms A, B, C, D, &c. If r be negative, the denominators become $1+r$, $(1+r)^2$, $(1+r)^3$, &c.

It has been obſerved, in the Meditationes, that in ſwift converging ſeries the ſeries P will converge more ſwiftly than the ſeries Q; in ſeries converging according to a geometrical ratio, ſometimes the one will converge more ſwift, and ſometimes the other. In other ſeries, which converge more ſlow, where moſt commonly r nearly $=1$, it cannot in general be ſaid, which of the ſerieſes will converge the ſwifteſt. The preceding remark, *viz.* the addition of the firſt terms of the ſeries, is neceſſary in moſt caſes of finding the ſums by ſerieſes of this kind.

It is not unworthy of obſervation, that in almoſt all caſes of infinite ſeries, the convergency depends on the roots of the given equations, which remark was firſt publiſhed in the Meditationes. For example: in finding approximates to the roots of given equations the convergency depends on how much the approximates given are more near to one root than to any other; and conſequently, when two or more roots or values of an unknown quantity are nearly equal, different rules are to be applied, which are improvements of the rule of falſe. This rule, and the above-mentioned obſervations were firſt given in the Meditationes Algebraicæ et Analyticæ, with ſeveral other additions on ſimilar ſubjects.

Many

Many more things concerning the ſummation of ſeries, which depend on fluxional, &c. equations, might be added; but I ſhall conclude this paper with congratulating myſelf, that ſome algebraical inventions publiſhed by me have been ſince thought not unworthy of being publiſhed by ſome of the greateſt mathematicians of this or any other age.

1ſt, In the year 1757, I ſent to the Royal Society the firſt edition of my Meditationes Algebraicæ: they were printed and publiſhed in the years 1760 and 1762, with Properties of Curve Lines, under the title of Miſcellanea Analytica, and a copy of them ſent to Mr. EULER in the beginning of the year 1763, in which was contained a reſolution of algebraical equations, not inferior, on account of its generality and facility, to any yet publiſhed (*viz.* $y = a\sqrt[n]{p} + b\sqrt[n]{p^2} + c\sqrt[n]{p^3} + \ldots\sqrt[n]{p^{n-1}}$). This reſolution was publiſhed by Mr. EULER in the Peterſburg Acts for the year 1764. Whether Mr. EULER ever received my book, I cannot pretend to ſay; nor is it material: for the fact is, that it was publiſhed by me in the year 1760 and 1762, and firſt by Mr. EULER in the year 1764. Mr. DE LA GRANGE and Mr. BEZOUT have aſcribed this reſolution to Mr. EULER, as firſt publiſhed in the year 1764, not having ſeen (I ſuppoſe) my Miſcell. Analyt. Mr. BEZOUT found from it ſome new equations, of which the reſolution is known, and applied it to the reduction of equations: more new equations are given, and the reſolution rendered more eaſy by me in the Philoſophical Tranſactions. 2d, In the above-mentioned Miſcell. Analyt. an equation is transformed into another, of which the roots are the ſquares of the differences of the roots of the given equation; and it is aſſerted in that book, that if the

co-efficients

co-efficients of the terms of the reſulting equations change continually from + to − and − to +, the roots of the given equation are all poſſible, otherwiſe not; and in a paper, inſerted by me in the Philoſophical Tranſactions for the year 1764, in which is found from this transformation, when there are none, two or four impoſſible roots contained in an algebraical equation of four or five dimenſions; it is obſerved, that there will be none or four, &c. impoſſible roots contained in the given equation, if the laſt term be + or −; and two, &c. on the contrary, if the laſt term be − or +. Theſe obſervations and tranſformation have been ſince publiſhed and explained in the Berlin Acts for the years 1767 and 1768, by Mr. DE LA GRANGE. 3d. In the Miſcell. Anal. an equation is transformed into another, whoſe roots are the ſquares, &c. of the roots of a given equation; and it is aſſerted, that there are at leaſt ſo many impoſſible roots contained in the given equation, as there are continual progreſſes in the reſulting equation from + to + and − to −. It is afterwards remarked, that theſe rules ſometimes find impoſſible roots when Sir ISAAC NEWTON's, and ſuch like rules, fail; and that Sir ISAAC NEWTON's, &c. will find them, when this rule fails. This rule may ſomewhat further be promoted by firſt changing the given equation, whoſe root is x, into another whoſe root is $\sqrt{-1}x$; but, in my opinion, the rule of HARRIOT's, which only finds whether there are impoſſible roots contained in a cubic equation or not, is to be preferred to theſe rules, which, in equations of any dimenſions, of which the impoſſible roots cannot generally be found from the rules, ſeldom find the true number. 4th, It is remarked, that rules which diſcover the true number of impoſſible roots require immenſe calculations, ſince they muſt neceſſarily find, when

the roots become equal. In order to this, in the Miſcell. Anal. there is found an equation, whoſe roots are the reciprocals of the differences of any two roots of the given equation; and from finding a quantity (π) greater than the greateſt root of the given, and $\left(\frac{1}{A}\right)$ greater than the greateſt root of the reſulting equation, and ſubſtituting π, $\pi - A$, $\pi - 2A$, &c. for x in the given equation; will always be found the true number of impoſſible roots. 5th, In the ſame book are aſſumed two equations ($nx^{n-1} - \overline{n-1}px^{n-2} + \overline{n-2}qx^{n-3} - $ &c. $= 0$ and $x^n - px^{n-1} + $ &c. $= w$), and thence deduced an equation, whoſe root is w, from which, in ſome caſes, can be found the number of impoſſible roots.

6. In the Miſcell. Anal. is given the law of a ſeries, and its demonſtration, which finds the ſum of the powers of the roots of a given equation from its co-efficients. Mr. EULER has ſince publiſhed the ſame in the Peterſburg Acts. Mr. DE LA GRANGE printed a property of this ſeries, alſo printed by me about the ſame time; *viz.* that if the ſeries was continued *in infinitum*, the powers would obſerve the ſame law as the roots, which indeed immediately follows from the ſeries itſelf; but from thence with the greateſt ſagacity he deduces the law of the reverſion of the ſeries ($y = a + bx + cx^2 + dx^3 +$ &c.): it has ſince been given in a different manner from ſimilar principles in the Medit. Analyt. 7. In the Miſcell. Analyt. the law of a ſeries is given for finding the ſum of all quantities of this kind ($\alpha^n \times \beta^n \times \gamma^n \times \delta^n \times$ &c. $+$ &c.) where $\alpha, \beta, \gamma, \delta$, &c. denote the roots of a given equation, from the powers of the roots of the given equation. This law, with a different notation, has been ſince publiſhed in the Paris Acts by Mr. VANDERMONDE; who indeed mentions that he

had heard, that a series for that purpose was contained in my book, but had not seen it. In the same book is given a method of finding the aggregates of any algebraical functions of each of the roots of given equations, which is somewhat improved in the latter editions. 8. In the same book are assumed $\frac{az^n+bz^{n-1}+\&c.}{pz^m+qz^{m-1}+\&c.}$ and $\frac{Az^n+Bz^{n-1}+\&c.}{pz^m+qz^{m-1}+\&c.}$, where z is any rational quantity whatever for x and y, the unknown quantities of a given equation of two or more dimensions. 9. In the Miscell. Analyt. a biquadratic ($x^4+2px^3=qx^2+rx+s$, of which no term is destroyed) is reduced to a quadratic ($x^2+px+n=\sqrt{p^2+2n+q}x+\sqrt{s+n^2}$;) and in the second edition of it, printed in the years 1767, 1768, 1769, and published in the beginning of the year 1770, the values of n are found $\frac{\alpha\beta+\gamma\delta}{2}$, $\frac{\alpha\gamma+\beta\delta}{2}$, and $\frac{\alpha\delta+\beta\gamma}{2}$; and the six values of $\sqrt{y^2+2n+q}$ respectively $\frac{\alpha+\beta-\gamma-\delta}{2}$, $\frac{\alpha+\gamma-\beta-\delta}{2}$, $\frac{\alpha+\delta-\beta-\gamma}{2}$, and their negatives; and the six values of $\sqrt{s+n^2}$ respectively $\frac{\alpha\beta-\gamma\delta}{2}$, $\frac{\alpha\gamma-\beta\delta}{2}$, $\frac{\alpha\delta-\beta\gamma}{2}$, and their negatives. 10. From a given biquadratic ($y^4+qy^2+ry+s=0$) by assuming $y^2+ay+b=v$ and a and b such quantities as to make the second and fourth terms of the resulting equations to vanish, there results an equation ($v^4+Av^2+B=0$) of the formula of a quadratic. Mr. DE LA GRANGE has ascribed this resolution to Mr. TSCHIRNHAUSEN; but in the Leipsic Acts the resolution of a cubic is given by Mr. TSCHIRNHAUSEN, but not of a biquadratic: his general design seems to be the extermination of all the terms.

11. Mr. EULER or Mr. DE LA GRANGE found, that if α be a root of the equation $x^n - 1 = 0$, where n is a prime number, $\alpha, \alpha^2, \alpha^3, \ldots, \alpha^{n-1}$, 1 will be (n) roots of it. More on a similar ſubject has been added in the laſt edition of the Medit. Algebr. 12. It is obſerved in the Miſcell. Analyt. that CARDAN's or SCIPIO FERREUS's reſolution of a cubic is a reſolution of three different cubic equations; and in the Medit. Algeb. 1770, the three cubics are given, and the rationale of the reſolution (for example: if α, β, and γ, be the roots of the cubic equation $x^3 + qx - r = 0$, then is given the function of the above roots, which are the roots of the reducing equation $z^6 - rz^3 = q^3$); and alſo the rationale of the common reſolution of biquadratics. 13. It is aſſerted in the Miſcell. that if the terms ($My^n + by^{n-1}x + cy^{n-2}x^2 +$ &c. and $Ny^m + By^{m-1}x + Cy^{m-2}x^2 +$ &c.) of two equations of n and m dimenſions, which contain the greateſt dimenſions of x and y have a common diviſor, the equation whoſe root is x or y, will not aſcend to $n \times m$ dimenſions; and if the equation, whoſe root is x or y, aſcends to $n \times m$ dimenſions, the ſum of its roots depends on the terms of n and $n-1$ dimenſions in the one, and m and $m-1$ dimenſions in the other equation, &c. It is alſo aſſerted, in the Miſcell. that if three algebraical equations of n, m, and r dimenſions contain three unknown quantities x, y, and z, the equation, whoſe root is x or y or z, cannot aſcend to more than $n \cdot m \cdot r$ dimenſions. 14. Mr. BEZOUT has given two very elegant propoſitions for finding the dimenſions of the equation whoſe root is x or y, &c.; where x, y, &c. are unknown quantities contained in two or more (b) algebraical equations of π, ρ, σ, &c. dimenſions, and in which ſome of the unknown quantities do not aſcend to the above π, ρ, σ, &c.

dimenſions

dimensions respectively. In demonstrating these propositions he uses one (amongst others) before given by me (*viz.* if an equation of n dimensions contains m unknown quantities, the number of different terms which may be contained in it will be $n+1 \cdot \frac{n+2}{2} \cdot \frac{n+3}{3} \ldots \frac{n+m}{m}$). In the Medit. 1770 there is given a method of finding in many cases the dimensions of the equation, whose root is x or y, &c.; from which one, if not both, of the above-mentioned cases may more easily be deduced, and others added. 15. In the Medit. 1770 is observed, that if there be n equations containing m unknown quantities, where n is greater than m, there will be $n-m$ equations of conditions, &c. 16. In the Miscell. is given and demonstrated the subsequent proposition; *viz.* if two equations contain two unknown quantities x and y, in which x and y are similarly involved; the equation, whose root is x or y will have twice the number of roots which the equation, whose root is $x+y$, x^2+y^2, &c. has. In the Medit. 1770, the same reasoning is applied to equations, which have two, three, four, &c. quantities similarly involved. 17. Mr. DE LA GRANGE has done me the honour to demonstrate my method of finding the number of affirmative and negative roots contained in a biquadratic equation. A demonstration of my rule for finding the number of affirmative, negative, and impossible roots contained in the equation $x^n + Ax^m + B = 0$ is also omitted, on account of its ease and length. From the Medit. the investigation of finding the true number of affirmative and negative roots appears to be as difficult a problem as the finding the true number of impossible roots; and it further appears, that the common methods in both cases can seldom be depended on. But their faults lie on different sides,

the one generally finds too many, the other too few. 18. In the Medit. 1770, from the number of impoſſible roots in a given equation ($x^n - px^{n-1} + \&c. = 0$) is found the number of impoſſible roots in an equation, whoſe roots (v) have any aſſignable relation to the roots of a given equation; and examples are given in the relation ($nx^{n-1} - \overline{n-1}px^{n-2} + \&c. = v$); and in an equation, whoſe roots are the ſquares of the differences of the roots of the given equation. 19. It is obſerved in the Medit. 1770, that in two or more equations, having two or more unknown quantities, the ſame irrationality will be contained in the correſpondent values of each of the unknown quantities, unleſs two or more values of one of them are equal, &c. The ſame obſervation is alſo applied to the coefficients of an equation deduced from a given equation. 20. In the Miſcell. was publiſhed a new method of exterminating, from a given equation, irrational quantities, by finding the the multipliers, which, multiplied into it, give a rational product. 21. In the Medit. 1770, are given the different reſolutions of a certain quantity $(a^s + rb^s)^{m+1}$ and $(a^s + rb^s)^{m+\frac{1}{2}}$ into quantities of the ſame kind. 22. Mr. DE LA GRANGE has very elegantly demonſtrated Mr. WILSON's celebrated property of prime numbers contained in my book. In the laſt edition of the Medit. the ſame property is demonſtrated, and ſome ſimilar ones added. 23. In the Miſcell. is given a method of finding all the integral correſpondent values of the unknown quantities of a given ſimple equation, having two or more unknown quantities; and, in the Medit. 1770, are given methods of reducing ſimple and other algebraical equations into one, ſo that ſome unknown quantities may be exterminated; and if the unknown quantities of the reſulting equations be integral or

rational,

rational, the unknown quantities exterminated may alſo be integral or rational. 24. In the Medit. are given rules for finding the different and correſpondent roots of an equation, whoſe reſolution is given. 25. Mr. DE LA GRANGE has recommended my new transformation of equations, publiſhed in the Miſcell. which perhaps is not leſs general nor elegant than any yet publiſhed; and in the Meditat. 1770 is given a method very uſeful in finding the co-efficients.

If either here, or in the preface to the Medit. Algebraicæ, I have aſcribed to myſelf any algebraical, or in the properties of curve lines any geometrical, or in the Medit. Analyt. any analytical invention, which has been before publiſhed by any other perſon, I can only plead ignorance of it, and ſhall on the very firſt conviction acknowledge it.

I muſt further add, that I have been able to carry my algebraical improvements into geometry; for from them, with ſome geometrical principles added, I have (unleſs I am deceived) deduced as many new properties of conic ſections and curve lines as have been publiſhed by any one ſince the great geometrician APOLLONIUS.

XXIX. *An Account of a remarkable Froſt on the 23d of* June, 1783. *In a Letter from the Rev. Sir* John Cullum, *Bart. F. R. S. and S. A. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 27, 1784.

DEAR SIR, Hardwick-houſe, Nov. 10, 1783.

WHEN I had the pleaſure of ſeeing you in London, in the autumn, and mentioned a froſt that happened in my neighbourhood on the 23d of laſt June, you expreſſed a deſire of receiving ſome particulars about it. I therefore now ſend you ſome memorandums which I made at the time.

About ſix o'clock, that morning, I obſerved the air very much condenſed in my chamber-window; and, upon getting up, was informed by a tenant, who lives cloſe to my houſe, that finding himſelf cold in bed, about three o'clock in the morning, he looked out at his window, and to his great ſurpriſe ſaw the ground covered with a white froſt: and I was afterwards aſſured, upon indubitable authority, that two men at Barton, about three miles off, ſaw between three and four o'clock that morning, in ſome ſhallow tubs, ice of the thickneſs of a crown-piece, and which was not melted before ſix.

This unſeaſonable froſt produced ſome remarkable effects. The ariſtæ of the barley, which was coming into ear, became brown and withered at their extremities, as did the leaves of the oats; the rye had the appearance of being mildewed; ſo that

that the farmers were alarmed for thoſe crops. The wheat was not much affected. The larch, Weymouth pine, and hardy Scotch fir, had the tips of their leaves withered; the firſt was particularly damaged, and made a ſhabby appearance the reſt of the ſummer. The leaves of ſome aſhes, very much ſheltered in my garden, ſuffered greatly. A walnut-tree received a ſecond ſhock (the firſt was from a ſevere froſt on the 26th of May) which completed the ruin of its crop. Cherry-trees, a ſtandard peach-tree, filbert and haſel-nut trees, ſhed their leaves plentifully, and littered the walks as in autumn. The barberry-buſh was extremely pinched, as well as the hypericum perforatum and hirſutum: as the two laſt are ſolſtitial, and rather delicate plants, I wondered the leſs at their ſenſibility; but was much ſurpriſed to find, that the vernal black-thorn and ſweet violet, the leaves of which one would have thought muſt have acquired a perfect firmneſs and ſtrength, were injured full as much. All theſe vegetables appeared exactly as if a fire had been lighted near them, that had ſhrivelled and diſcoloured their leaves;

——— *penetrabile frigus adurit.*

At the time this havock was made among ſome of our hardy natives, the exotic mulberry-tree was very little affected; a fig-tree, againſt a north-weſt wall, remained unhurt, as well as the vine, on the other ſide, though juſt coming into bloſſom. I ſpeak of my own garden, which is high; for in the low ones about Bury, that is but a mile off, the fig-trees, in particular, were very much cut: and, in general, all thoſe gardens ſuffer more by froſt than mine.

Some weather, that was cold for the time of year, had preceded this froſt. On the 21ſt the thermometer had, at no time of the day, riſen to 60°; on the 22d, at ten at night, it

had ſunk to 50°. On the laſt day, and on the 23d, diſappeared that dry haze, which had taken place ſome days before, and continued to blot out the face of the ſun for ſo long a time afterwards. After ſun-ſet on the 24th it appeared again, and the next day the leaves of many vegetables were covered with a clammy ſweetneſs.

The above ſlight notes were taken in my garden and its environs; and I wiſh they may afford you the ſmalleſt entertainment. If you ſhould think them worth the attention of the Royal Society, diſpoſe of them accordingly. So ſevere a froſt, at ſo advanced a ſeaſon, is certainly not one of the leaſt remarkable among the atmoſpherical phænomena of this year.

I remain, dear Sir,

Your much obliged and faithful ſervant,

JOHN CULLUM.

XXX. *On a new Method of preparing a Teſt Liquor to ſhew the Preſence of Acids and Alkalies in chemical Mixtures.* By Mr. James Watt, *Engineer*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 27, 1784.

THE ſyrop of violets was formerly the teſt of the point of ſaturation of mixtures of acids and alkalies, which was principally uſed; but ſince the late improvements in chemiſtry it has been found not to be ſufficiently accurate, and the infuſion of tourneſol, or of an artificial preparation called litmus, have been ſubſtituted in the place of it.

The infuſion of litmus is blue, and becomes red with acids. It is ſenſible to the preſence of one grain of common oil of vitriol, though it be mixed with 100000 grains of water; but as this infuſion does not change its colour on being mixed with alkaline liquors, in order to diſcover whether a liquor be neutral or alkaline, it is neceſſary to add ſome vinegar to the litmus, ſo as juſt to turn the infuſion red, which will then be reſtored to its blue colour, by being mixed with any alkaline liquor. The blue infuſion of litmus is alſo a teſt of the preſence of fixed air in water, with which it turns red, as it does with other acids.

The great degree of ſenſibility of this teſt would leave very little reaſon to ſearch for any other, were there reaſon to believe that it is always a teſt of the exact point of ſaturation of

acids and alkalies, which the following fact ſeems to call in queſtion.

I have obſerved, that a mixture of phlogiſticated nitrous acid with an alkali will appear to be acid, by the teſt of litmus, when other teſts, ſuch as the infuſion of the petals of the ſcarlet roſe, of the blue iris, of violets, and of other flowers, will ſhew the ſame liquor to be alkaline, by turning green ſo very evidently as to leave no doubt.

At the time I made this diſcovery, the ſcarlet roſes and ſeveral other flowers, whoſe petals change their colour by acids and alkalies, were in flower. I ſtained paper with their juices, and found that it was not affected by the phlogiſticated nitrous acid, except in ſo far as it acted the part of a neutralizing acid; but I found alſo, that paper, ſtained in this manner, was by no means ſo eaſily affected by acids of any kind as litmus was, and that in a ſhort time it loſt much of that degree of ſenſibility it poſſeſſed. Having occaſion in winter to repeat ſome experiments, in which the phlogiſticated nitrous acid was concerned, I found my ſtained paper almoſt uſeleſs. I was, therefore, obliged to ſearch for ſome ſubſtitute among the few vegetables which then exiſted in a growing ſtate; of theſe I found the red cabbage (*braſſica rubra*) to furniſh the beſt teſt, and in its freſh ſtate to have more ſenſibility both to acids and alkalies than litmus, and to afford a more deciſive teſt, from its being naturally blue, turning green with alkalies, and red with acids; to which is joined the advantage of its not being affected by phlogiſticated nitrous acid any farther than it acts as a real acid.

To extract the colouring matter, take thoſe leaves of the cabbage, which are freſheſt, and have moſt colour; cut out the larger ſtems, and mince the thin parts of the leaves very ſmall; then digeſt them in water, about the heat of 120 degrees, for

a few hours, and they will yield a blue liquor, which, if uſed immediately as a teſt, will be found to poſſeſs great ſenſibility. But, as this liquor is very ſubject to turn acid and putrid, and to loſe its ſenſibility, when it is wanted to be preſerved for future uſe the following proceſſes ſucceed the beſt.

1. After having minced the leaves, ſpread them on paper, and dry them in a gentle heat; when perfectly dry, put them up in glaſs bottles well corked; and when you want to uſe them, acidulate ſome water with vitriolic acid, and digeſt, or infuſe, the dry leaves in it until they give out their colour; then ſtrain the liquor through a cloth, and add to it a quantity of fine whiting or chalk, ſtirring it frequently until it becomes of a true blue colour, neither inclining to green nor purple; as ſoon as you perceive that it has acquired this colour, filter it immediately, otherwiſe it will become greeniſh by longer ſtanding on the whiting.

This liquor will depoſite a ſmall quantity of gypſum, and by the addition of a little ſpirit of wine will keep good for ſome days, after which it will become a little putrid and reddiſh. If too much ſpirit is added, it deſtroys the colour. If the liquor is wanted to be kept longer, it may be neutralized by means of a fixed alkali inſtead of chalk.

2. But as none of theſe means will preſerve the liquor long without requiring to be neutralized afreſh, juſt before it is uſed; and as the putrid and acid fermentation which it undergoes, and perhaps the alkalies or ſpirit of wine mixed with it, ſeem to leſſen its ſenſibility; in order to preſerve its virtues while it is kept in a liquid ſtate, ſome freſh leaves of the cabbage, minced as has been directed, may be infuſed in a mixture of vitriolic acid and water, of about the degree of acidity of vinegar; and it may be neutralized, as it is wanted, either by means of chalk,

or

or of the fixed or volatile alkali. But it is neceſſary to obſerve, that if the liquor has an exceſs of alkali, it will ſoon loſe its colour, and become yellow, from which ſtate it cannot be reſtored; therefore care ſhould be taken to bring it very exactly to a blue, and not to let it verge towards a green *.

3. By the ſame proceſs I have made a red infuſion of violets, which, on being neutralized, forms at preſent a very ſenſible teſt; but how long it will preſerve its properties I have not yet determined. Probably the coloured infuſions of other flowers may be preſerved in the ſame manner, by the antiſeptic power of the vitriolic acid, ſo as to loſe little of their original ſenſibility. Paper, freſh ſtained with theſe teſts in their neutral ſtate, has ſufficient ſenſibility for many experiments; but the alum and glue which enter into the preparation of writing-paper ſeem in ſome degree to fix the colour; and paper which is not ſized becomes ſomewhat tranſparent, when wetted, which renders ſmall changes of colour imperceptible; ſo that where accuracy is required, the teſt ſhould be uſed in a liquid ſtate †.

* Since writing the above, I have found, that the infuſions of red cabbages and of various flowers in water acidulated by means of vitriolic acid, are apt to turn mouldy in the ſummer ſeaſon, and alſo that the moulding is prevented by the addition of ſpirits of wine. The quantity of ſpirit which is neceſſary for this purpoſe I have not been able to aſcertain; but I add it by little at a time, until the progreſs of the moulding is prevented.

† I have found, that the petals of the ſcarlet roſe, and thoſe of the pink-coloured lychnis, treated in this manner, afford very ſenſible teſts.

XXXI. *An Account of a new Plant, of the Order of* Fungi. *By* Thomas Woodward, *Eſq*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 10, 1784.

Plantæ novæ Deſcriptio — an Genus novum?

Radices paucæ; tenues; albidæ.

Volva ovata; duplex, mucilagine interpoſita; ſubalbida.

Stipes, e volva interiore ſurgens, ſublignoſus; cavus; cortice lacerato veſtitus; ſubfuſcus.

Capitulum, ſtipitis ſummitati inſidens, reflexum; ſubtus campanulatum, glabrum; ſuperne pulverulentum, et, e pulveris craſſitie, globiforme; volvæ ruptæ ſummitatem, minime adhærentem, in ſe gerens.

Pulvis ſphæricus; ſemipellucidus; luteo-fuſcus.

THIS extraordinary vegetable production ariſes from a volva, which is buried ſix or eight inches deep in dry ſandy banks; and, conſequently, it is extremely difficult to detect it in its earlieſt ſtate. At its firſt appearance above ground, the powdery head is covered with a looſe campanulated cap, which does not adhere by any the ſmalleſt filaments; and which, I ſuppoſe, to be the upper part of the volva, as both always appear ragged when taken up. When the plant is taken up immediately on its appearing above ground, the ſtem is about ſix or eight inches long; and, as well as the volva, replete with

with mucilage, making it much heavier than when it has attained its full growth. This is the ſtate to which the deſcription given above refers. The duſt is now perfectly formed, and is diſperſed by the ſlighteſt touch, or by the wind. A great alteration ſoon takes place, as it now proceeds very rapidly, and in a few days attains the ſummit of its growth, which is from nine to fifteen inches, more than half being generally buried in the ground. The ſtem becomes woody, though hollow, the bark ſtill more ragged, and the whole plant much lighter, both volva and ſtem being now quite dry, and free from mucilage. The wind and ſhowers ſoon diſperſe the greateſt part of the duſt; and at length the ſtalk appears with a naked, coriaceous, campanulated pileus, and conſiderably bleached, in colour and appearance not unlike a dry ſtalk of hemp. In this ſtate ſome of them are now to be found (Aug. 28, 1783) with plants of this year riſing near them.

Mr. HUMPHREYS, of Norwich, who firſt found this very extraordinary plant, met with it only in the ſtate laſt deſcribed, and without diſcovering the volva; ſo that no judgement of it could be formed. It has been taken by ſome perſons for a decayed or abortive agaric; but that opinion could not be maintained by any one who had ſeen it in its recent ſtate.

I firſt met with it in February or March 1783 in its dry and withered ſtate; but as it was ſuſpected, though with little appearance of reaſon, to be a decayed Agaricus procerus, I wiſhed to examine the root carefully, in order to obſerve whether it was bulbous. The bulb of the Agaricus procerus is ſcarcely hidden under the ſurface, and I was much ſurpriſed at the depth to which I was obliged to ſearch for the root of this

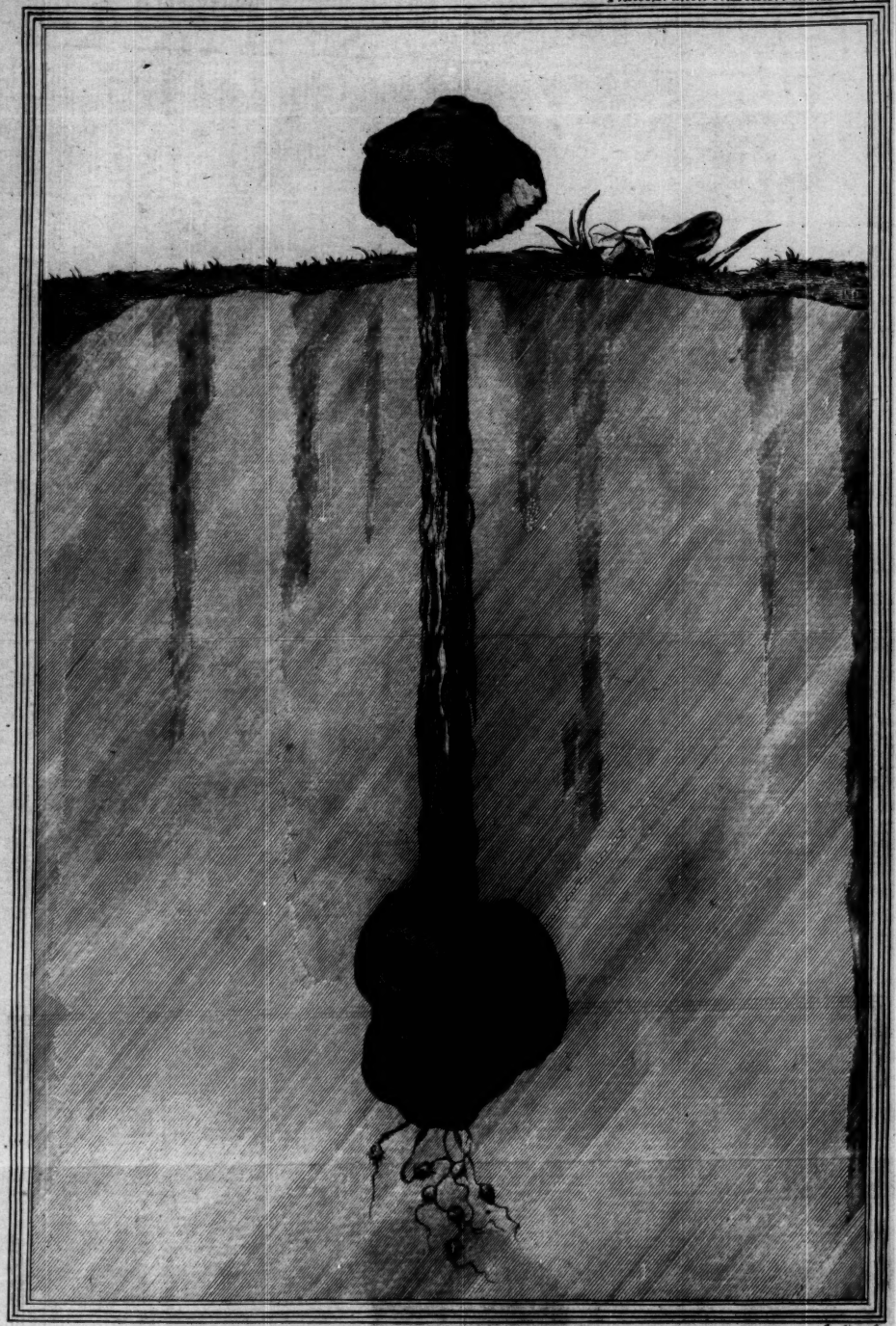
this plant; at length, however, removing the earth carefully to the depth of seven or eight inches, I met with it, and to my great pleasure and surprise, on raising the plant, I discovered the volva, which was so unlike the fugitive one of the agaric, that I was immediately convinced it must be something new.

An account of this was directly sent to Mr. DICKSON, of Covent-Garden, an able botanist, and diligent enquirer after the class Cryptogamia. Mr. DICKSON, who had before seen it in the state in which it was found by Mr. HUMPHREYS, but could make nothing of it, though thoroughly convinced it was no agaric, immediately requested that I would watch the spot, and endeavour to detect the plant in its earliest appearance. I communicated this to my neighbour Mr. STONE, a most diligent and skilful botanist, who first restored the Lycoperdon coliforme; and we determined to examine the spot carefully together, from the month of August downward. About the middle of August we first discovered a plant just arisen, which was sent to Mr. DICKSON, and a full description of which is before given; but though we have daily visited the spot since, we have never been able to find it again in so young a state; for so rapid appears to be its growth, that we have found plants of two or three inches height above the ground, the stems of which had lost part of their mucilage, where the day before none had been visible. We have three or four times attempted to discover the volva in its earliest state, by removing the earth carefully near the old stems of the preceding year; but this has been without success: and there is little hope of succeeding in it, as the volva lies very deep in the ground, and the plant arises at such various times.

This plant agrees with the genus Phallus in its volva, which has a double coat replete with mucilage; and its ſtipes crowned with a reflexed pileus. But it more nearly approaches the genus Lycoperdon, by its head covered with a thick duſt, contained in a ſubſtance of a ſpongy appearance, and by the form of the duſt, which agrees perfectly with that of moſt of the true lycoperdons, when examined in the microſcope. To this genus it muſt at preſent probably be referred, though the total want of an exterior coat prevents its agreeing with it ſo perfectly as it ought.

The Mucor * ſepticus of HUDSON and LIGHTFOOT (Mucor ovatus of SCHÆFFER); the Mucor * butyraceus of SCHÆFFER (194.), not taken notice of by either HUDSON or LIGHTFOOT, but which I have often found here; and the Lycoperdon * epidendrum of LIGHTFOOT, which I ſuppoſe to be what HUDSON calls Lycoperdon epiphyllum, as he has referred to the ſame plate of SCHÆFFER (193. Mucor fragiformis); have all ſome affinity with the fructification of this plant; and the more ſo, if we ſuppoſe the head to be at firſt covered with a mucilage, which afterwards turns to a duſt; but this will hardly be admitted, as the plant ſent to Mr. DICKSON had the duſt perfectly formed, though the volva and ſtem were both replete with mucilage. But we cannot admit it to agree with any of theſe laſt mentioned plants, as they have all an exterior coat, though very fugitive, of which this ſeems entirely deſtitute. We may add, that they are all very fugitive productions;

* I cannot help obſerving that, in my opinion, HALLER has done more rightly in making theſe into a new genus (Filago), than our botaniſts, who have jumbled them with the genera Lycoperdon and Mucor, to which they have no great affinity any more than the Sphæria of HALLER, likewiſe very improperly ranked with the Lycoperdons and Clavariæ.



Basire sc.

whereas this, though ſoon arriving at maturity, is of a woody and permanent ſtructure.

P. S. In a letter Mr. DICKSON received from Mr. WOODWARD, Feb. 12, 1784, he informs him, that he is quite convinced by ſome late obſervations, that the above-mentioned plant frequently comes to a ſtate of perfection before it reaches the ſurface. The only difference to be obſerved is, that the duſt in that caſe is of a darker colour, which he ſuppoſes is owing to its not being expoſed to the air.

XXXII. *Experiments to inveſtigate the Variation of Local Heat.* *By* James Six, *Eſq.*; *communicated by the Rev.* Francis Wollaſton, *LL.B. F. R. S.*

Read June 10, 1784.

BEING deſirous of inveſtigating the variation of local heat, I made the following experiments.

On the 4th of September, 1783, I placed thermometers in three different ſtations; one on the top of the high tower of Canterbury Cathedral, about 220 feet from the ground; another at the bottom of the ſame tower, at about 110 feet; and a third in my own garden *, not more than ſix feet from the ground. They were all carefully expoſed to the open air in a ſhady northern aſpect; the loweſt was as little liable to be affected by the reflection of the ſun's rays as the elevation would permit, the ſecond ſtill leſs, and the higheſt not at all. They continued unremoved in their ſeveral places, where I viſited them daily for the ſpace of three weeks, and minuted down the greateſt degree of heat and cold that happened each day and night in their reſpective ſtations †.

* This garden is ſituate not far from the Cathedral, at the extremity of the buildings on the north ſide of the city.

† The thermometers here made uſe of were conſtructed to ſhew the greateſt degree of heat and cold which happened in the obſerver's abſence (deſcribed Phil. Tranſ. vol. LXXII. part I.), which rendered them particularly convenient on this occaſion. They had hung together for ſome time, and ſeldom differed half a degree from each other.

By

By theſe obſervations it appears, ſee Table I. that, notwithſtanding ſome irregularities, the heat of the days at the loweſt ſtation always exceeded that at the middle, and ſtill more the heat at the upper ſtation. As in many inſtances the higher regions of the atmoſphere have been found to be colder than the lower, and the thermometer in the garden was more liable to be heated by the reflection of the ſun's rays from the earth than the upper ones, a difference of this kind might have been expected. But I was greatly ſurpriſed to find the cold of the night at the loweſt, not only equal to, but, very frequently, exceeding the cold at the higher ſtations. As I wiſhed to know, whether theſe variations would continue the ſame in the winter, when the weather was colder; and whether a thermometer, placed at ſome diſtance from the city, having an elevation equal to that on the top of the Cathedral tower, would agree with it; on the 19th of December, 1783, I diſpoſed the three thermometers in the following manner: one in my garden; one on the top of the high tower, as before; and the third on the top of St. Thomas's Hill, about a mile diſtant from the city, where, at fifteen feet from the ground, it was nearly level with that on the Cathedral tower. Table II. contains the obſervations that were then made *. The weather at this time proving cold, favoured the experiment; and I now found the ſeveral thermometers nearly agreeing with each other in the day-time: but in the night, the cold at the lower ſtation exceeded the cold at the higher ones rather more than it did in the month of September, when the weather was warmer.

* The few omiſſions in this Table were occaſioned by the ſeverity of the cold preventing my attending at a proper time the thermometers, which were at a conſiderable diſtance from each other.

At

At the time of taking these thermometrical observations, I likewise noted the different dispositions of the atmosphere in other respects: such as the pressure, moisture, and dryness of the air; force and direction of the winds; quantity of rain; whether the appearances of the sky were clear or cloudy, &c. as I apprehended the local variation of the thermometers might, in a certain degree, correspond with some particular change in the state of the atmosphere.

The event answered my expectation in a singular manner in respect to the nocturnal variation; for it generally happened, that when the sky was dark and cloudy, whatever was the condition of the atmosphere with relation to the other particulars above enumerated, the thermometers agreed pretty nearly with each other; but, on the contrary, whenever the sky became clear, the cold of the night at the lowest station in the garden constantly exceeded the cold at the top of the Cathedral tower, where the instrument was placed 220 feet from the ground, entirely exposed to the open air, wind, dews, and rain, in a shady northern aspect.

The local variations in the day-time seemed to be regulated by the general degree of heat only, without being affected by any other particular disposition of the atmosphere, or the clearness or cloudyness of the sky, as the nocturnal variations were. In the month of September, when the glasses rose from 60° to 70°, the heat at the lower station constantly exceeded the heat at the upper station; and in some measure proportionally, as the weather was hotter *. In December and January, when

* As the heat at the lower station exceeded the heat at the upper ones, when the weather was hot; and equally so, whenever the sky was cloudy, as well as when it was clear; it appears, that the glass at the lower station was not materially affected by the reflection of the sun's rays from the earth, as at first I apprehended it would be.

from

from below 30° they ſeldom roſe to 40°, the local variation in the day-time nearly ceaſed, or was found in very ſmall degrees inclining ſometimes one way, ſometimes the other.

That the clearneſs of the ſky ſhould contribute to the coolneſs of the air in the night, is not at all ſurpriſing; but that, whenever the ſky becomes clear, the cold ſhould ſeem to ariſe from the earth, and be found in the greateſt degree, as long as it continues clear, in the loweſt ſituation, ſeems a little extraordinary: this, however, appeared to be the caſe, both in the warmer as well as in the colder weather, during the whole time theſe obſervations were taken, and remarkably ſo on the following days. On the firſt of January the weather was cold, the ſky cloudy, the glaſſes in the night were at 20°, and in the day at 34°: the wind which had been at S.E. the day before, changed in the evening to S. and brought on a thaw. On the ſecond of January clouds and miſty rain darkened the ſky all day; the wind blew briſkly at S.W.; the glaſſes in the night were at 32°½, in the day at 40°. On the third of January the clouds and rain continued, the weather growing ſtill warmer; wind at S.W. by S.; the glaſſes in the night were at 36°, in the day at 45°½. Theſe three days the weather gradually became warmer; and, while the ſky remained darkened by clouds, all the glaſſes in their ſeveral ſtations nearly agreed with each other. About noon, on the third of January, the ſky becoming clear, the air grew cooler; and going into my garden, about eight o'clock in the evening, I perceived the ſurface of the ground, which had been wet by the rain in the forenoon, began to be frozen. Looking immediately at the thermometer, I ſaw the mercury at 33°½; and obſerving a piece of wet linen hanging near the glaſs, not five feet from the ground, I took it into my hand, and found it not in the leaſt frozen; by which it appeared,

that

that the degree of cold which had frozen the ſurface of the ground, had not then aſcended to the glaſs, nor to the linen, and conſequently had not been communicated to the air five or ſix feet above the earth. The next day I found, as I expected, a conſiderable local variation; the index for the cold of the night in the garden being at 32°, that on the hill being at 35°¼, and that on the top of the tower at 37°¾ *. Probably the weather did not continue clear the whole night; if it had, it is likely the degrees of cold would have been found proportionally greater at every ſtation. On the morning of the 4th there fell a miſty rain, which continued only till noon, when the ſky became clear again, and continued ſo till the 7th; during which time the nocturnal heights of the thermometers differed conſiderably from each other; but on the ſky's becoming cloudy, the local variation ceaſed.

Thermometrical obſervations, made under the ſame circumſtances in reſpect to the ſeaſon of the year, place, and ſituation †, may probably be liable to ſimilar local varia-

* It is remarkable, that the thermometer on St. Thomas's hill did not vary ſo much from that in the garden, as that did which was on the Cathedral tower, although theſe two elevated glaſſes were within three feet of a perfect level with each other; the variations, however, as often as they happened, inclined the ſame way. The reaſon of this might probably be, that although the glaſs on the hill was at an equal altitude with that on the tower, in reſpect to the ground on which the Cathedral ſtands: yet the former was only 15 feet, while the latter was 220 feet from the ground.

† Situation in regard to hill or valley. The valley in which Canterbury ſtands is at that place about a mile in breadth, opening to the N.E.; the hills on either ſide do not riſe very ſudden, nor very high; the river Stour, divided into branches, paſſes through the city, and, about fourteen miles below, empties itſelf into the ſea, which waſhes the coaſt from the NN.W. round by the E. to the S.; diſtant from the city at different places from ſix to ſixteen miles.

tions: to thoſe who make them, the reſult of theſe experiments may be of ſome uſe. If convenient opportunity offered, I ſhould be glad, by the aſſiſtance of friends, to try the local difference of heat and cold in more diſtant, as well as more elevated, ſituations.

By experiments of this kind it may poſſibly in ſome meaſure be found, how far evaporations from the earth, at certain times, or vapours aſcending, deſcending, or meeting, in different parts of the atmoſphere, may increaſe or diminiſh the heat of the air in thoſe places: or whether different degrees of heat and cold (ſubject however to change) may not be found in different ſtrata of air, or vapour, floating in different parts of the atmoſphere; or in what degree and proportion, the cold increaſes at different altitudes and in different ſeaſons of the year: whether the cold, which is known to be very intenſe in the ſummer time on the tops of high mountains, receives a proportional increaſe, or be not leſs ſubject to variety by the return of winter and ſummer, night and day, than what we experience in the plains below.

March 10, 1784. JAMES SIX.

TABLE I.

The greateſt daily variation of heat and cold in the atmoſphere, from the 4th to the 24th of September, 1783, taken from three different ſtations, and compared together. One thermometer placed on a tower in Canterbury, 220 feet from the ground; another at the bottom of the ſame tower, 110; and a third in a garden, about ſix feet from the ground. N. B. The nocturnal degrees of cold belong to the night immediately preceding the day to the date of which they are placed.

	Greateſt degree of cold in the night.					Greateſt degree of heat in the day.					
	Thermometer in the garden.	Thermometer at the bottom of the tower.	Thermometer on the top of the tower.	Difference of garden from bottom of tower.	Difference of garden from top of tower.	Thermometer in the garden.	Thermometer at the bottom of the tower.	Thermometer on the top of the tower.	Difference of garden from bottom of tower.	Difference of garden from top of tower.	
Sept.	°	°	°	°	°	°	°	°	°	°	
4	50½	—	51	—	−0½	66	61	61	+5	+5	Morning ſtill and foggy; wind began to blow in the forenoon at S.W.; clouds and rain in the afternoon and night; bar. 29.3.
5	48	47½	47½	+0½	+0½	62½	61½	61½	+1	+1	Morning cloudy; heavy rain; clear in the afternoon; wind high at W.N.W.; bar. 29.3.
6	48½	50	50½	−1½	−2	66½	65½	64½	+1½	+2	Morning rained a little; wind very high at S.W. moſt part of the day; bar. 29.5.
7	48	49½	49½	−1½	−1½	63½	62½	63	+1	+0½	Morning clear; continued ſo moſt part of the day; wind very high at W.; bar. 29.8.
8	50	51	51	−1	−1	66	62½	62	+3½	+4	Sometimes clear, ſometimes cloudy; wind very high at W.; bar. 29.9.
9	55½	55½	55½	—	—	65	63	62½	+2	+2½	Morning cloſe and cloudy; clear at noon; wind briſk at S.W.; bar. 29.5.
10	45	47	47½	2	−2½	63½	59½	59½	+4	+4	Morning and great part of the day clear; wind high at S.W.; evening clear; bar. 29.6.
11	42	45	45½	−3	−3½	63½	62	60½	+1½	+3	Morning clear; cloudy about noon; briſk wind at S.; evening ſtill; bar. 29.8.
12	52½	53½	54	−1	−1½	69	66½	65	+2½	+4	Morning cloudy; wind high at S.; evening ſtill and clear; bar. 29.4.
13	45	48	48½	−3	−3½	65	62	62	+3	+3	Morning clear; a little rain at noon; cloudy afternoon; wind briſk at S.; bar. 29.0.
14	57½	57	57	+0½	+0½	68½	66½	64½	+2	+4	Morning cloudy; moiſt warm air; wind briſk at S.W.; miſty rain; cloudy evening; bar. 29.8.
15	57	57	58	—	−1	70	68½	66	+1½	+4	Morning cloudy; wind moderate S.W.; in the evening changed to N.; bar. 29.5.
16	52½	53	52½	−0½	—	65½	62½	61	+3	+4½	Morning hazy; thin clouds all day; little wind at N.E.; cloſe and warm; bar. 29.8.
17	51½	51½	51	—	+0½	62½	61	60½	+1½	+2	Dull and hazy moſt part of the day; little breeze of wind at N.E.; bar. 30.1.
18	57	57	57	—	—	62½	62	61	+0½	+1½	Very dull all day; wind briſk at N.E.; bar 29.9.
19	53	54½	55½	−1½	−2½	70	67½	67	+2½	+3	Morning clear; a little rain in the afternoon; wind S.S.E.; bar. 29.6.
20	56	56	55½	—	+0½	64½	63	61	+1½	+3½	Cloudy all day, with rain and wind S.W.; clear at night; bar. 29.4.
21	44½	47½	48½	−3	−4	63½	61	60½	+2½	+3	Morning clear; wind at S.W.; clear moſt part of the day; wind S.; bar. 29.8.
22	56	57	57	−1	−1	59	58	58	+1	+1	Rain moſt part of the day; evening hazy; wind S.W.; bar. 29.6.
23	50	49	50	+1	—	63	59½	59½	+3½	+3½	Morning ſtill and miſty; a little ſhower in the afternoon, clear all the reſt of the day; wind S.W.; bar. 29.6.
24	43½	46	46	−2½	−2½	63	59½	58½	+3½	+4½	Clear all day; wind W. and N.W.; bar. 29.8.

TABLE II.

The greateſt daily variation of heat and cold in the atmoſphere from the 20th of December, 1783, to the 8th of January, 1784, taken from three different ſtations, and compared together. One thermometer placed on a tower in Canterbury, 220 feet from the ground; another on a hill, a mile diſtant, but on the ſame level with that on the tower; a third in a garden, about ſix feet from the ground. N. B. The nocturnal degrees of cold belong to the night immediately preceding the day to the date of which they are placed.

	Greateſt degree of cold in the night.					Greateſt degree of heat in the day.					
	Thermometer in the garden.	Thermometer on the hill.	Thermometer on the tower.	Difference of garden from hill.	Difference of garden from tower.	Thermometer in the garden.	Thermometer on the hill.	Thermometer on the tower.	Difference of garden from hill.	Difference of garden from tower.	
Dec. 20	20°	25°	25¼°	−5°	−5¼°	39½°	37¾°	39°	+1¾°	−0½°	Evening preceding clear; morning clear; wind briſk at W.; cloudy at noon; air very moiſt; barometer at 29.9.
21	29¾	30¼	32	−1	−2¼	37¾	38½	38	−0¾	−0¼	Morning clear on the hill; fog in the city; little wind at N.W.; air moiſt: cloudy at noon; a little ſnow; bar. 29.8.
22	22	24½	25½	−2½	−3½	34	34¾	36	−0¾	−2	Morning ſlight fog in the city; little wind at N.W.; air moiſt: cloudy at noon; a little ſnow; bar. 29.7.
23	31¾	31¾	32	—	−0¼	39¾	40¼	37½	−0½	+2¼	Dark and cloudy all day; wind briſk at S.W.; air moiſt: evening foggy; bar. 29.8.
24	31¾	33	34½	−1¼	−2¾	43¼	42	41½	+1¼	+2¼	Morning rainy; clear at noon; cloudy late in the evening; wind at N.; bar. 29.6.
25	26	27	—	−1	—	36	35¼	—	+0¾	—	Morning cloudy; wet miſt; wind briſk at E.; towards evening rain mixed with ſnow; night clear: bar. 29.2.
26	26½	26½	28	—	−1½	33¾	34	36	−0½	−2½	Morning foggy; a little ſnow about noon; evening clear in the zenith; little wind at N.; bar. 28.9.
27	25	26½	28	−1½	−3	36	34½	35½	+1½	−0½	Morning clear; cloudy at noon; little ſnow in the evening; moderate breeze of wind at N.E.; bar. 29.3.
28	30	29	29½	+1	+0½	31¼	30	33½	+1¼	−2¼	Morning dark and cloudy; wind very briſk at N.E.; air dry, and felt very cold; bar. 29.3.
29	21	21½	22	−0½	−1	24¾	24¾	—	−0¾	—	Morning hazy; dry miſty air; wind very cold and briſk at S.E. by E.; bar. 29.7.
30	15½	15	16½	+0½	−1	22	21½	21¾	+0½	+0¼	Morning clear in the zenith; dry miſty fog below; wind very cold and briſk at S.E. bar. 29.7.
31	12¼	11¾	13	+0½	−0¾	21½	—	21¾	—	—	Sometimes clear, ſometimes cloudy; wind briſk at S.E.; evening rain with ſnow; wind S. very high in the night; bar. 29.4.
Jan. 1	20	—	20	—	—	34	—	33¾	—	+0¼	Wet miſt all day; wind moderate at W.; bar. 29.4.
2	32½	32	32½	+0½	—	40	39½	40	+0½	—	Morning thick fog and miſty rain; wind S.E.; afternoon and evening very high at S.W. with rain; bar. 29.6.
3	36	35½	36	+0½	—	45½	45	45½	+0½	—	Morning rainy; wind S.W. by W.; clear at noon; afternoon and evening very clear and ſtill; wind S.W.; bar. 29.6.
4	32	35¼	37¾	−3¼	−5¾	46½	44¾	45½	+1¾	+1	Morning miſty rain; wind high at S.; evening very clear and ſtill; bar. 29.8.
5	26½	29¾	31	−3	−4½	36½	35¾	—	+1	—	Morning very clear; little wind at N.E. evening very clear and ſtill; bar. 30.1.
6	21½	26	27½	−4½	−6	31	30¼	—	+0¾	—	Morning very clear; moderate breeze of wind at SE.; evening very clear and ſtill; bar. 30.2.
7	16	19	20½	−3	−4½	29	27½	27½	+1½	+1½	Morning very clear; cloudy about noon; evening dark and cloudy; little wind S.E.; bar. 30.0.
8	26	25½	25½	+0½	+0½	32	32	31½	—	+0½	Morning dark and cloſe; very dark all; wind W.N.W.; bar. 29.8.

XXXIII. *Account of ſome Obſervations tending to inveſtigate the Conſtruction of the Heavens.* By William Herſchel, *Eſq. F. R. S.*

Read June 17, 1784.

IN a former paper I mentioned, that a more powerful inſtrument was preparing for continuing my reviews of the heavens. The teleſcope I have lately completed, though far inferior in ſize to the one I had undertaken to conſtruct when that paper was written, is of the Newtonian form, the object ſpeculum being of 20 feet focal length, and its aperture $18\frac{7}{10}$ inches. The apparatus on which it is mounted is contrived ſo as at preſent to confine the inſtrument to a meridional ſituation, and by its motions to give the right-aſcenſion and declination of a celeſtial object in a coarſe way; which, however, is ſufficiently accurate to point out the place of the object, ſo that it may be found again. It will not be neceſſary to enter into a more particular deſcription of the apparatus, ſince the account I have now the honour of communicating to the Royal Society regards rather the performance of the teleſcope than its conſtruction.

It would, perhaps, have been more eligible to have waited longer, in order to complete the diſcoveries that ſeem to lie within the reach of this inſtrument, and are already, in ſome reſpects, pointed out to me by it. By taking more time I ould undoubtedly be enabled to ſpeak more confidently of the

interior conſtruction of the heavens, and its various *nebulous and ſidereal ſtrata* (to borrow a term from the natural hiſtorian) of which this paper can as yet only give a few outlines, or rather hints. As an apology, however, for this prematurity, it may be ſaid, that the end of all diſcoveries being communication, we can never be too ready in giving facts and obſervations, whatever we may be in reaſoning upon them.

Hitherto the ſidereal heavens have, not inadequately for the purpoſe deſigned, been repreſented by the concave ſurface of a ſphere, in the center of which the eye of an obſerver might be ſuppoſed to be placed. It is true, the various magnitudes of the fixed ſtars even then plainly ſuggeſted to us, and would have better ſuited the idea of an expanded firmament of three dimenſions; but the obſervations upon which I am now going to enter ſtill farther illuſtrate and enforce the neceſſity of conſidering the heavens in this point of view. In future, therefore, we ſhall look upon thoſe regions into which we may now penetrate by means of ſuch large teleſcopes, as a naturaliſt regards a rich extent of ground or chain of mountains, containing ſtrata variouſly inclined and directed, as well as conſiſting of very different materials. A ſurface of a globe or map, therefore, will but ill delineate the interior parts of the heavens.

It may well be expected, that the great advantage of a large aperture would be moſt ſenſibly perceived with all thoſe objects that require much light, ſuch as the very ſmall and immenſely diſtant fixed ſtars, the very faint nebulæ, the cloſe and compreſſed cluſters of ſtars, and the remote planets.

On applying the teleſcope to a part of the *via lactea*, I found that it completely reſolved the whole whitiſh appearance into ſmall ſtars, which my former teleſcopes had not light enough

to effect. The portion of this extenſive tract, which it has hitherto been convenient for me to obſerve, is that immediately about the hand and club of Orion. The glorious multitude of ſtars of all poſſible ſizes that preſented themſelves here to my view was truly aſtoniſhing; but, as the dazzling brightneſs of glittering ſtars may eaſily miſlead us ſo far as to eſtimate their number greater than it really is, I endeavoured to aſcertain this point by counting many fields, and computing, from a mean of them, what a certain given portion of the milky way might contain. Among many trials of this ſort I found, laſt January the 18th, that ſix fields, promiſcuouſly taken, contained 110, 60, 70, 90, 70, and 74 ſtars each. I then tried to pick out the moſt vacant place that was to be found in that neighbourhood, and counted 63 ſtars. A mean of the firſt ſix gives 79 ſtars for each field. Hence, by allowing 15 minutes of a great circle for the diameter of my field of view, we gather, that a belt of 15 degrees long and two broad, or the quantity which I have often ſeen paſs through the field of my teleſcope in one hour's time, could not well contain leſs than fifty thouſand ſtars, that were large enough to be diſtinctly numbered. But, beſides theſe, I ſuſpected at leaſt twice as many more, which, for want of light, I could only ſee now and then by faint glittering and interrupted glimpſes.

The excellent collection of nebulæ and cluſters of ſtars which has lately been given in the *Connoiſſance des Temps* for 1783 and 1784, leads me next to a ſubject which, indeed, muſt open a new view of the heavens. As ſoon as the firſt of theſe volumes came to my hands, I applied my former 20-feet reflector of 12 inches aperture to them; and ſaw, with the greateſt pleaſure, that moſt of the nebulæ, which I had an opportunity of examining in proper ſituations, yielded to the

force of my light and power, and were reſolved into ſtars. For inſtance, the 2d, 5, 9, 10, 12, 13, 14, 15, 16, 19, 22, 24, 28, 30, 31, 37, 51, 52, 53, 55, 56, 62, 65, 66, 67, 71, 72, 74, 92, all which are ſaid to be nebulæ without ſtars, have either plainly appeared to be nothing but ſtars, or at leaſt to contain ſtars, and to ſhew every other indication of conſiſting of them entirely. I have examined them with a careful ſcrutiny of various powers and light, and generally in the meridian. I ſhould mention, that five of the above, *viz.* the 16th, 24, 37, 52, 67, are called cluſters of ſtars containing nebuloſity; but my inſtrument reſolving alſo that portion of them which is called nebulous into ſtars of a much ſmaller ſize, I have placed them into the above number. To theſe may be added the 1ſt, 3d, 27, 33, 57, 79, 81, 82, 101, which in my 7, 10, and 20-feet reflectors ſhewed a mottled kind of nebuloſity, which I ſhall call reſolvable; ſo that I expect my preſent teleſcope will, perhaps, render the ſtars viſible of which I ſuppoſe them to be compoſed. Here I might point out many precautions neceſſary to be taken with the very beſt inſtruments, in order to ſucceed in the reſolution of the moſt difficult of them; but reſerving this at preſent too extenſive ſubject for a future opportunity, I proceed to ſpeak of the effects of my laſt inſtrument with regard to nebulæ.

My preſent purſuits, as I obſerved before, requiring this teleſcope to act as a fixed inſtrument, I found it not convenient to apply it to any other of the nebulæ in the *Connoiſſance des Temps* but ſuch as came in turn; nor, indeed, was it neceſſary to take any particular pains to look for them, it being utterly impoſſible that any one of them ſhould eſcape my obſervation when it paſſed the field of view of my teleſcope. The few which I have already had an opportunity of examining, ſhew plainly that

thoſe moſt excellent French aſtronomers, Meſſ. MESSIER and MECHAIN, ſaw only the more luminous part of their nebulæ; the feeble ſhape of the remainder, for want of light, eſcaping their notice. The difference will appear when we compare my obſervation of the 98th nebula with that in the *Connoiſſance des Temps* for 1784, which runs thus: "Nébuleuſe ſans étoile, "d'une lumière extrêmement foible, au deſſus de l'aile boréale "de la Vierge, ſur le parallèle et près de l'étoile N° 6. cin- "quième grandeur, de la chevelure de Bérénice, ſuivant "FLAMSTEED. M. MECHAIN la vit le 15 Mars, 1781." My obſervation of the 30th of December, 1783, is thus: A large, extended, fine nebula. Its ſituation ſhews it to be M. MESSIER's 98th; but from the deſcription it appears, that that gentleman has not ſeen the whole of it, for its feeble branches extend above a quarter of a degree, of which no notice is taken. Near the middle of it are a few ſtars viſible, and more ſuſpected. My field of view will not quite take in the whole nebula. See fig. 1. tab. XVII. Again, N° 53. "Nébuleuſe ſans étoiles, "découverte au-deſſous et près de la chevelure de Bérénice, à "peu de diſtance de l'étoile quarante-deuxieme de cette conſtel- "lation, ſuivant FLAMSTEED. Cette nébuleuſe eſt ronde et "apparente, &c." My obſervation of the 170th Sweep runs thus: A cluſter of very cloſe ſtars; one of the moſt beautiful objects I remember to have ſeen in the heavens. The cluſter appears under the form of a ſolid ball, conſiſting of ſmall ſtars, quite compreſſed into one blaze of light, with a great number of looſe ones ſurrounding it, and diſtinctly viſible in the general maſs. See fig. 2.

When I began my preſent ſeries of obſervations, I ſurmiſed, that ſeveral nebulæ might yet remain undiſcovered, for want of ſufficient light to detect them; and was, therefore, in hopes

of

of making a valuable addition to the cluſters of ſtars and nebulæ already collected and given us in the work before referred to, which amount to 103. The event has plainly proved that my expectations were well founded: for I have already found 466 new nebulæ and cluſters of ſtars, none of which, to my preſent knowledge, have been ſeen before by any perſon; moſt of them, indeed, are not within the reach of the beſt common teleſcopes now in uſe. In all probability many more are ſtill in reſerve; and as I am purſuing this track, I ſhall make them up into ſeparate catalogues, of about two or three hundred at a time, and have the honour of preſenting them in that form to the Royal Society.

A very remarkable circumſtance attending the nebulæ and cluſters of ſtars is, that they are arranged into ſtrata, which ſeem to run on to a great length; and ſome of them I have already been able to purſue, ſo as to gueſs pretty well at their form and direction. It is probable enough, that they may ſurround the whole apparent ſphere of the heavens, not unlike the milky way, which undoubtedly is nothing but a ſtratum of fixed ſtars. And as this latter immenſe ſtarry bed is not of equal breadth or luſtre in every part, nor runs on in one ſtraight direction, but is curved and even divided into two ſtreams along a very conſiderable portion of it; we may likewiſe expect the greateſt variety in the ſtrata of the cluſters of ſtars and nebulæ. One of theſe nebulous beds is ſo rich, that, in paſſing through a ſection of it, in the time of only 36 minutes, I detected no leſs than 31 nebulæ, all diſtinctly viſible upon a fine blue ſky. Their ſituation and ſhape, as well as condition, ſeems to denote the greateſt variety imaginable. In another ſtratum, or perhaps a different branch of the former, I have ſeen double and treble nebulæ, variouſly arranged; large ones

with

with ſmall, ſeeming attendants; narrow but much extended, lucid nebulæ or bright daſhes; ſome of the ſhape of a fan, reſembling an electric bruſh, iſſuing from a lucid point; others of the cometic ſhape, with a ſeeming nucleus in the center; or like cloudy ſtars, ſurrounded with a nebulous atmoſphere; a different ſort again contain a nebuloſity of the milky kind, like that wonderful, inexplicable phænomenon about θ Orionis; while others ſhine with a fainter, mottled kind of light, which denotes their being reſolvable into ſtars. See fig. 3. &c. But it would be too extenſive at preſent to enter more minutely into ſuch circumſtances, therefore I proceed with the ſubject of nebulous and ſidereal ſtrata.

It is very probable, that the great ſtratum, called the milky way, is that in which the ſun is placed, though perhaps not in the very center of its thickneſs. We gather this from the appearance of the Galaxy, which ſeems to encompaſs the whole heavens, as it certainly muſt do if the ſun is within the ſame. For, ſuppoſe a number of ſtars arranged between two parallel planes, indefinitely extended every way, but at a given conſiderable diſtance from each other; and, calling this a ſidereal ſtratum, an eye placed ſomewhere within it will ſee all the ſtars in the direction of the planes of the ſtratum projected into a great circle, which will appear lucid on account of the accumulation of the ſtars; while the reſt of the heavens, at the ſides, will only ſeem to be ſcattered over with conſtellations, more or leſs crowded, according to the diſtance of the planes or number of ſtars contained in the thickneſs or ſides of the ſtratum.

Thus, in fig. 16. (tab. XVIII.) an eye at S within the ſtratum *a b*, will ſee the ſtars in the direction of its length *a b*, or height *c d*, with all thoſe in the intermediate ſituations, projected into the

lucid circle ACBD; while thoſe in the ſides *m v*, *n w*, will be ſeen ſcattered over the remaining part of the heavens at MVNW.

If the eye were placed ſomewhere without the ſtratum, at no very great diſtance, the appearance of the ſtars within it would aſſume the form of one of the leſs circles of the ſphere, which would be more or leſs contracted to the diſtance of the eye; and if this diſtance were exceedingly increaſed, the whole ſtratum might at laſt be drawn together into a lucid ſpot of any ſhape, according to the poſition, length, and height of the ſtratum.

Let us now ſuppoſe, that a branch, or ſmaller ſtratum, ſhould run out from the former, in a certain direction, and let it alſo be contained between two parallel planes extended indefinitely onwards, but ſo that the eye may be placed in the great ſtratum ſomewhere before the ſeparation, and not far from the place where the ſtrata are ſtill united. Then will this ſecond ſtratum not be projected into a bright circle like the former, but will be ſeen as a lucid branch proceeding from the firſt, and returning to it again at a certain diſtance leſs than a ſemi-circle.

Thus, in the ſame figure, the ſtars in the ſmall ſtratum *p q* will be projected into a bright arch at PRRP, which, after its ſeparation from the circle CBD, unites with it again at P.

What has been inſtanced in parallel planes may eaſily be applied to ſtrata irregularly bounded, and running in various directions; for their projections will of conſequence vary according to the quantities of the variations in the ſtrata and the diſtance of the eye from the ſame. And thus any kind of curvatures, as well as various different degrees of brightneſs, may be produced in the projections.

From appearances then, as I obſerved before, we may infer, that the ſun is moſt likely placed in one of the great ſtrata of the fixed ſtars, and very probably not far from the place where ſome ſmaller ſtratum branches out from it. Such a ſuppoſition will ſatisfactorily, and with great ſimplicity, account for all the phænomena of the milky way, which, according to this hypotheſis, is no other than the appearance of the projection of the ſtars contained in this ſtratum and its ſecondary branch. As a farther inducement to look on the Galaxy in this point of view, let it be conſidered, that we can no longer doubt of its whitiſh appearance ariſing from the mixed luſtre of the numberleſs ſtars that compoſe it. Now, ſhould we imagine it to be an irregular ring of ſtars, in the center nearly of which we muſt then ſuppoſe the ſun to be placed, it will appear not a little extraordinary, that the ſun, being a fixed ſtar like thoſe which compoſe this imagined ring, ſhould juſt be in the center of ſuch a multitude of celeſtial bodies, without any apparent reaſon for this ſingular diſtinction; whereas, on our ſuppoſition, every ſtar in this ſtratum, not very near the termination of its length or height, will be ſo placed as alſo to have its own Galaxy, with only ſuch variations in the form and luſtre of it, as may ariſe from the particular ſituation of each ſtar.

Various methods may be purſued to come to a full knowledge of the ſun's place in the ſidereal ſtratum, of which I ſhall only mention one as the moſt general and moſt proper for determining this important point, and which I have already begun to put in practice. I call it *Gaging the Heavens*, or the *Star-Gage*. It conſiſts in repeatedly taking the number of ſtars in ten fields of view of my reflector very near each other, and by adding their ſums, and cutting off one decimal on the right, a mean of the contents of the heavens, in all the parts which

re thus gaged, is obtained. By way of example, I have joined a ſhort table, extracted from the gages contained in my journal, by which it appears, that the number of ſtars increaſes very faſt as we approach the Via Lactea.

N. P. D. 92 to 94°.	
R. A.	Gage.
15 40	9.4
15 22	10.6
15 47	10.6
16 8	12.1
16 25	13.6
16 37	18.6

N. P. D. 78 to 80.	
R. A.	Gage.
11 16	3.1
12 31	3.4
12 44	4.6
12 49	3.9
13 5	3.8
14 30	3.6

Thus, in the parallel from 92 to 94 degrees north polar diſtance, and R. A. 15 h. 10′, the ſtar-gage runs up from 9.4 ſtars in the field to 18.6 in about an hour and a half; whereas in the parallel from 78° to 80° north polar diſtance, and R. A. 11, 12, 13, and 14 hours, it very ſeldom riſes above 4. We are, however, to remember, that with different inſtruments the account of the gages will be very different, eſpecially on our ſuppoſition of the ſituation of the ſun in a ſtratum of ſtars. For, let *a b*, fig. 17. be the ſtratum, and ſuppoſe the ſmall circle *g h l k* to repreſent the ſpace into which, by the light and power of a given teleſcope, we may penetrate; and let GHLK be the extent of another portion, which we are enabled to viſit by means of a larger aperture and power; it is evident, that the gages with the latter inſtrument will differ very much in their account of ſtars contained at MN, and at KG or LH; when with the former they will hardly be affected by the change from *m n* to *k g* or *l h*. And this accounts for what a celebrated author ſays concerning the effects of teleſcopes,

ſcopes, by which we muſt underſtand the beſt of thoſe that are in common uſe *.

It would not be ſafe to enter into an application of theſe, and ſuch other gages as I have already taken, till they are ſufficiently continued and carried all over the heavens. I ſhall, therefore, content myſelf with juſt mentioning that the ſituation of the ſun will be obtained, from conſidering in what manner the ſtar-gage agrees with the length of a ray revolving in ſeveral directions about an aſſumed point, and cut off by the bounds of the ſtratum. Thus, in fig. 18. let S be the place of an obſerver; S*r r*, S*r r*, lines in the planes *r* S *r*, *r* S *r*, drawn from S within the ſtratum to one of the boundaries, here repreſented by the plane AB. Then, ſince neither the ſituation of S, nor the form of the limiting ſurface AB, is given, we are to aſſume a point, and apply to it lines proportional to the ſeveral gages that have been obtained, and at ſuch angles from each other as they may point out; then will the termination of theſe lines delineate the boundary of the ſtratum, and conſequently manifeſt the ſituation of the ſun within the ſame. But to proceed.

If the ſun ſhould be placed in the great ſidereal ſtratum of the milky way, and, as we have ſurmiſed above, not far from

* On voit avec les téleſcopes des étoiles dans toutes les parties du ciel, à peu près comme dans la voie lactée, ou dans les nébuleuſes. On ne ſauroit douter qu'une partie de l'éclat et de la blancheur de la voie lactée, ne provienne de la lumière des petites étoiles qui s'y trouvent en effet par millions; cependant, avec les plus grands téleſcopes, on n'en diſtingue pas aſſés, et elles n'y ſont pas aſſés rapprochées les unes des autres pour qu'on puiſſe attribuer à celles qu'on diſtingue la blancheur de la voie lactée, ſi ſenſible à la vue ſimple. L'on ne ſauroit donc prononcer que les étoiles ſoient la ſeule cauſe de cette blancheur, quoique nous ne connoiſſions aucune manière ſatisfaiſante de l'expliquer. Aſtr. M. De la Lande, §. 838.

the

the branching out of a ſecondary ſtratum, it will very naturally lead us to gueſs at the cauſe of the probable motion of the ſolar ſyſtem: for the very bright, great node of the Via Lactis, or union of the two ſtrata about Cepheus and Caſſiopeia, and the Scorpion and Sagittarius, points out a conflux of ſtars manifeſtly quite ſufficient to occaſion a tendency towards that node in any ſtar ſituated at no very great diſtance; and the ſecondary branch of the Galaxy not being much leſs than a ſemi-circle ſeems to indicate ſuch a ſituation of our ſolar ſyſtem in the great undivided ſtratum as the moſt probable.

What has been ſaid in a former paper on the ſubject of the ſolar motion ſeems alſo to ſupport this ſuppoſed ſituation of the ſun; for the apex there aſſigned lies nearly in the direction of a motion of the ſun towards the node of the ſtrata. Beſides, the joining ſtratum making a pretty large angle at the junction with the primary one, it may eaſily be admitted, that the motion of a ſtar in the great ſtratum, eſpecially if ſituated conſiderably towards the ſide fartheſt from the ſmall ſtratum, will be turned ſufficiently out of the ſtraight direction of the great ſtratum towards the ſecondary one. But I find myſelf inſenſibly led to ſay more on this ſubject than I am as yet authoriſed to do; I will, therefore, return to thoſe obſervations which have ſuggeſted the idea of celeſtial ſtrata.

In my late obſervations on nebulæ I ſoon found, that I generally detected them in certain directions rather than in others; that the ſpaces preceding them were generally quite deprived of their ſtars, ſo as often to afford many fields without a ſingle ſtar in it; that the nebulæ generally appeared ſome time after among ſtars of a certain conſiderable ſize, and but ſeldom among very ſmall ſtars; that when I came to one nebula, I generally found ſeveral more in the neighbourhood; that afterwards

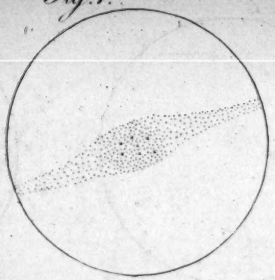
wards a conſiderable time paſſed before I came to another parcel; and theſe events being often repeated in different altitudes of my inſtrument, and ſome of them at a conſiderable diſtance from each other, it occurred to me, that the intermediate ſpaces between the ſweeps might alſo contain nebulæ; and finding this to hold good more than once, I ventured to give notice to my aſſiſtant at the clock, "to prepare, ſince I ex-"pected in a few minutes to come at a ſtratum of the nebulæ, "finding myſelf already" (as I then figuratively expreſſed it) "on nebulous ground." In this I ſucceeded immediately; ſo that I now can venture to point out ſeveral not far diſtant places, where I ſhall ſoon carry my teleſcope, in expectation of meeting with many nebulæ. But how far theſe circumſtances of vacant places preceding and following the nebulous ſtrata, and their being as it were contained in a bed of ſtars, ſparingly ſcattered between them, may hold good in more diſtant portions of the heavens, and which I have not yet been able to viſit in any regular manner, I ought by no means to hazard a conjecture. The ſubject is new, and we muſt attend to obſervations, and be guided by them, before we form general opinions.

Before I conclude, I may, however, venture to add a few particulars about the direction of ſome of the capital ſtrata or their branches. The well known nebula of Cancer, viſible to the naked eye, is probably one belonging to a certain ſtratum, in which I ſuppoſe it to be ſo placed as to lie neareſt to us. This ſtratum I ſhall call that of Cancer. It runs from ε Cancri towards the ſouth over the 67 nebula of the *Connoiſſance des Temps*, which is a very beautiful and pretty much compreſſed cluſter of ſtars, eaſily to be ſeen by any good teleſcope, and in which I have obſerved above 200 ſtars at once in the field of view

view of my great reflector, with a power of 157. This cluſter appearing ſo plainly with any good, common teleſcope, and being ſo near to the one which may be ſeen by the naked eye, denotes it to be probably the next in diſtance to that within the quartile formed by γ, δ, η, θ; from the 67th nebula the ſtratum of Cancer proceeds towards the head of Hydra; but I have not yet had time to trace it farther than the equator.

Another ſtratum, which perhaps approaches nearer to the ſolar ſyſtem than any of the reſt, and whoſe ſituation is nearly at rectangles to the great ſidereal ſtratum in which the ſun is placed, is that of Coma Berenices, as I ſhall call it. I ſuppoſe the Coma itſelf to be one of the cluſters in it, and that, on account of its nearneſs, it appears to be ſo ſcattered. It has many capital nebulæ very near it; and in all probability this ſtratum runs on a very conſiderable way. It may, perhaps, even make the circuit of the heavens, though very likely not in one of the great circles of the ſphere: for, unleſs it ſhould chance to interſect the great ſidereal ſtratum of the milky way before-mentioned, in the very place in which the ſun is ſtationed, ſuch an appearance could hardly be produced. However, if the ſtratum of Coma Berenices ſhould extend ſo far as (by taking in the aſſiſtance of M. MESSIER's and M. MECHAIN's excellent obſervations of ſcattered nebulæ, and ſome detached former obſervations of my own) I apprehend it may, the direction of it towards the north lies probably, with ſome windings, through the great Bear onwards to Caſſiopeia; thence through the girdle of Andromeda and the northern Fiſh, proceeding towards Cetus; while towards the ſouth it paſſes through the Virgin, probably on to the tail of Hydra and the head of Centaurus. But, notwithſtanding I have already fully aſcertained the exiſtence and direction of this ſtratum for more

Fig. 1.



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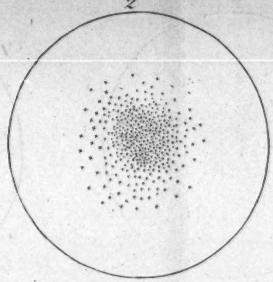

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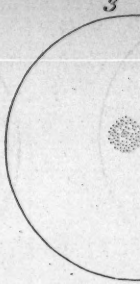

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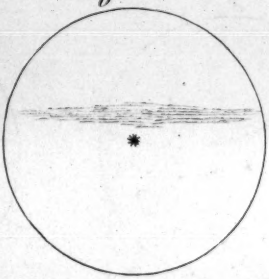

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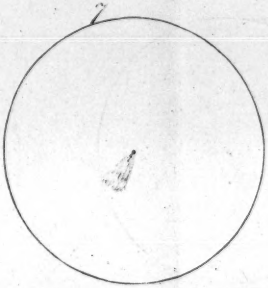

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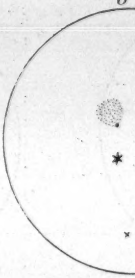

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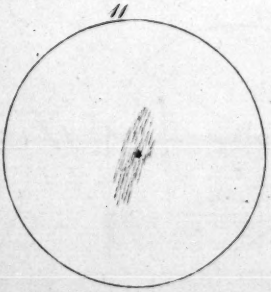

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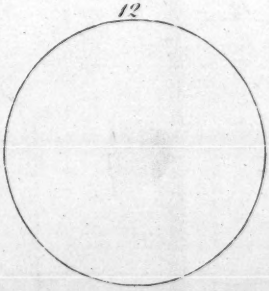

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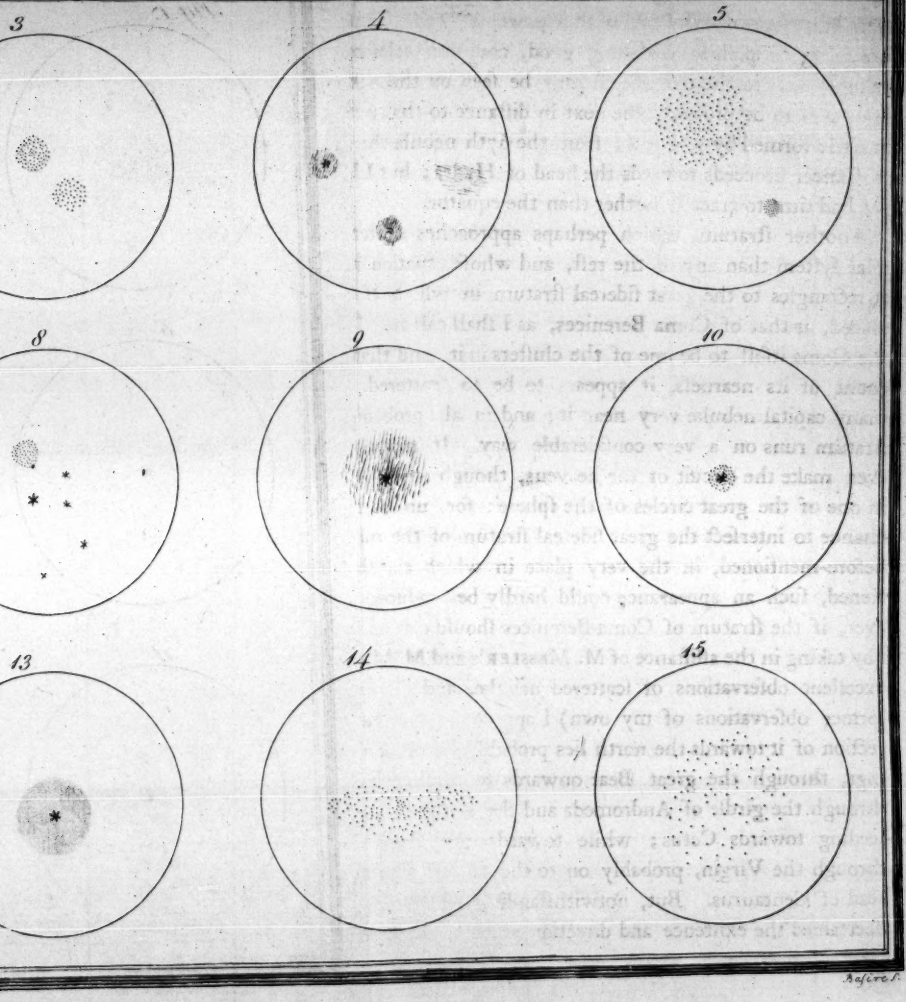
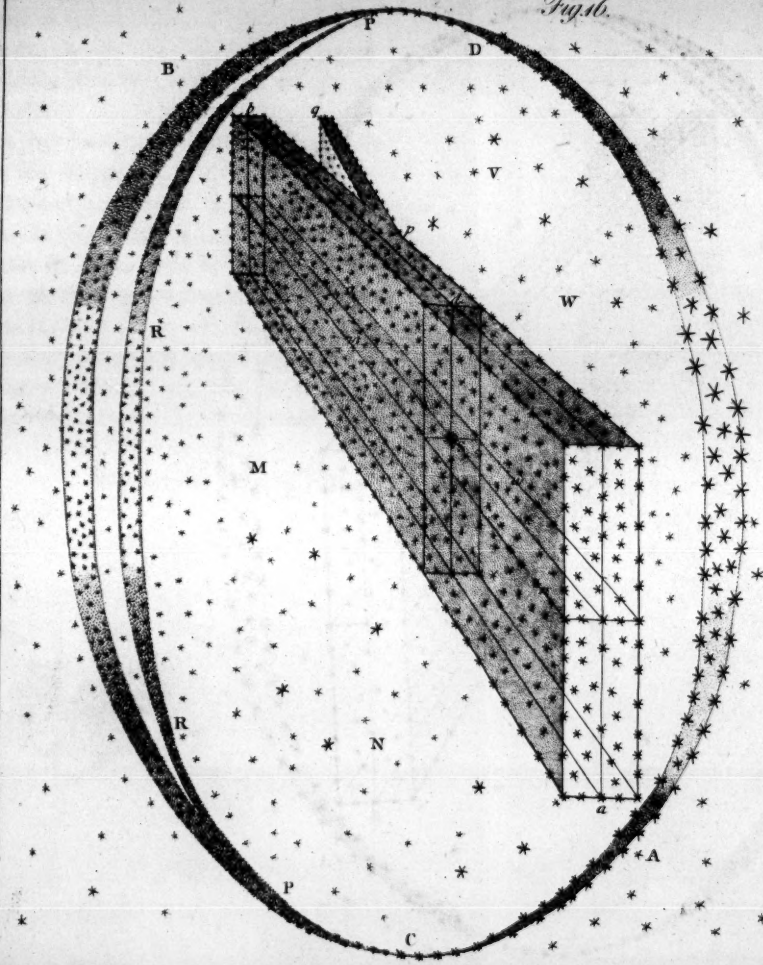


Basire sc.

Fig. 16.



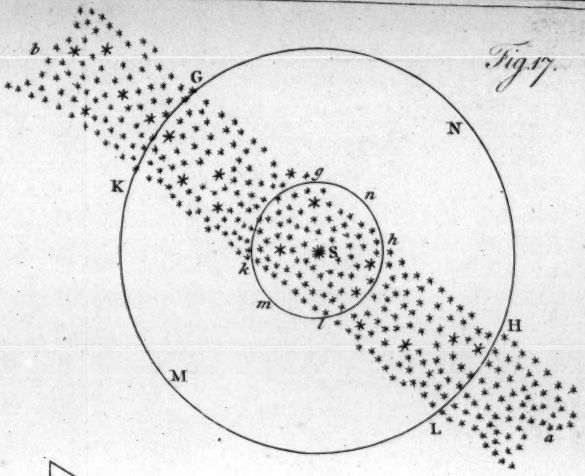


Fig. 17.

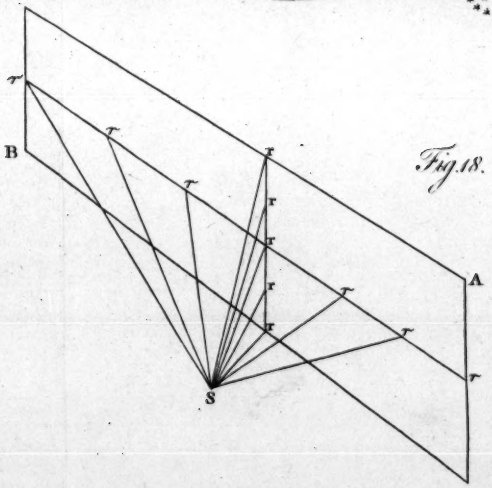
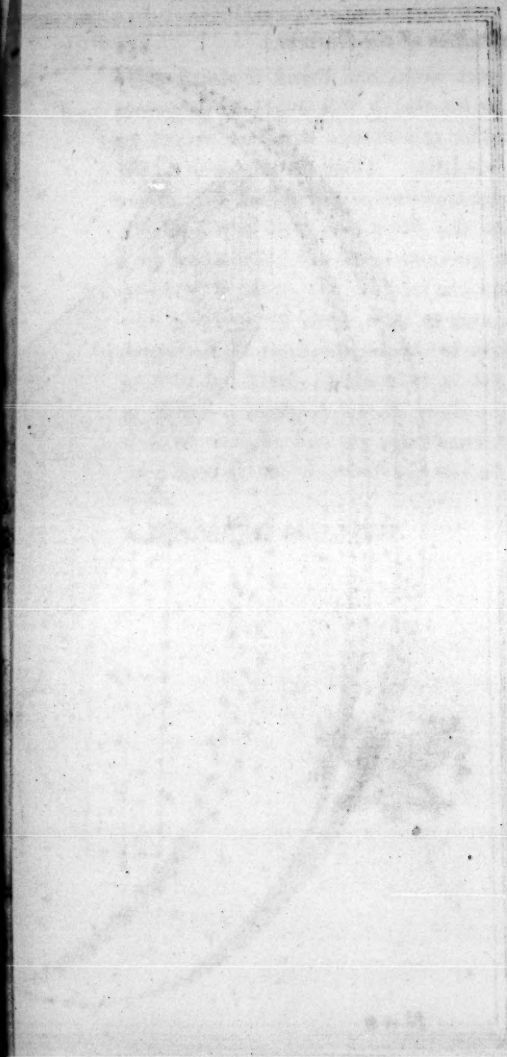


Fig. 18.



than 30 degrees of a great circle, and found it almoſt every where equally rich in fine nebulæ, it ſtill might be dangerous to proceed in more extenſive conjectures, that have as yet no more than a precarious foundation. I ſhall therefore wait till the obſervations in which I am at preſent engaged ſhall furniſh me with proper materials for the diſquiſition of ſo new a ſubject. And though my ſingle endeavours ſhould not ſucceed in a work that ſeems to require the joint effort of every aſtronomer, yet ſo much we may venture to hope, that, by applying ourſelves with all our powers to the improvement of teleſcopes, which I look upon as yet in their infant ſtate, and turning them with aſſiduity to the ſtudy of the heavens, we ſhall in time obtain ſome faint knowledge of, and perhaps be able partly to delineate, *the Interior Conſtruction of the Univerſe.*

Datchet near Windſor,
April, 1784. WILLIAM HERSCHEL.

XXXIV. *An Account of a new Species of the* Bark-Tree, *found in the Iſland of St. Lucia. By Mr.* George Davidſon; *communicated by* Donald Monro, *M. D. Phyſician to the Army, F. R. S.*

Read June 24, 1784.

DR. DONALD MONRO.

SIR,

HAVING received from my correſpondent Mr. DAVIDSON, ſurgeon, in the iſland of St. Lucia, ſome Bark, the product of that iſland, which is ſaid to poſſeſs the virtues of the Jeſuit's Bark, and in a much ſmaller doſe, I ſhall eſteem it a favour if you will lay before the Royal Society the ſpecimen which I have ſent to you with this letter, together with Mr. DAVIDSON's account of it, if you think they merit that honour.

I have examined the dried ſpecimens very carefully. They are not ſo well preſerved as I could wiſh; but I have ſince ſeen much finer in the poſſeſſion of Sir JOSEPH BANKS, who has done me the honour to favour me with the following character, as moſt diſtinctive of it from the other ſpecies of Cinchona already deſcribed, which he gave me an opportunity of examining.

It

It is undoubtedly a Cinchona, but not the Cinchona officinalis of LINNÆUS; for it differs from it eſſentially in its bark in ſeveral particulars. It has an emetic quality not common to the true bark, breaks more woody and ſplintery, and is far more nauſeous to the taſte. Its decoction is of a dull Burgundy colour; and its extract reſembles more the bitter of Gentian than that of the Quinquina. I have procured four ounces of it from half a pound of the Bark boiled in water, and herewith ſend to you a ſmall ſpecimen.

The drawings, which accompany this letter, are exact copies of the ſpecimens which I received; I therefore hope they will not be thought unworthy the acceptance of the Royal Society.

I have the honour to be, &c.

Henrietta-ſtreet, Nov. 6, 1783. G. WILSON.

Botanic character of the Bark-Tree of St. Lucia.

"Cinchona floribus paniculatis, glabris; laciniis linearibus, "tubo longioribus; ſtaminibus exſertis; foliis ellipticis, glabris."

Extract of a Letter from Mr. GEORGE DAVIDSON, *dated* St. Lucia, July 15, 1783.

IT is now about four years ſince Mr. ALEXANDER ANDERSON diſcovered in the woods, near the Grand Cul de Sac, ſome trees reſembling, in the botanical characters, the

true Quinquina of LINNÆUS. He brought the bark, flowers, and ſeeds, to Dr. YOUNG of the General Hoſpital, and trial was made of it there; but not being ſufficiently dried, its ſtrong emetic and purgative qualities prevented its exhibition.

The publication of Dr. SAUNDERS, which I received about two months ago, mentioning the introduction of a ſpecies of bark of a redder colour, and poſſeſſing greater powers than the bark formerly in uſe, induced us here to try the bark of this country. Dr. YOUNG had by him ſome that was collected in General GRANT's time: on account of the length of time it had been kept, and its being ſufficiently dried, he has met with all the ſucceſs he could wiſh.

It is manifeſtly more aſtringent than the bark, and the bitter is likewiſe more durable on the palate.

Hitherto I have generally uſed the cold infuſion, either in lime or ſimple water, in the proportion of one ounce to three pints of the water. I have likewiſe given it in ſubſtance from twenty to thirty grains; but never exceeded the laſt quantity, for I never found the ſtomach able to retain more than twenty grains.

Joined with the Canella alba, it forms in ſpirits an agreeable and elegant tincture. I have made a tincture from the ſeeds, which are infinitely ſtronger in taſte than the bark itſelf.

{Signed} GEO. DAVIDSON.

Mr.

Mr. George Davidſon's *account of the* Bark-Tree *of the iſland of* St. Lucia.

THE Bark-Tree of this iſland is nearly about the ſize of the cherry-tree, ſeldom thicker than the thigh, and tolerably ſtraight; the wood is light and porous, without any of the bitterneſs and aſtringency of the bark itſelf.

It delights in a ſhady ſituation, the north-weſt aſpect of hills, under larger trees; and is generally to be found about the middle of an hill, near ſome running water.

The leaves are large, oblong, oppoſite, and plain, preſerving (as well as the flowers and ſeeds) the bitter taſte of the bark.

In the beginning of the rainy ſeaſon (June), the tree puts forth its flowers in ſmall tufts; at firſt they are white, but afterwards turn purpliſh. The ſtamina are five in number, with a ſingle ſtyle. The germen is oblong, bilocular, and furrowed on each ſide. The ſeeds are many, and of the winged kind. The corolla is monopetalous, with its mouth divided into five long ſegments.

The ſoil in general where it grows is a ſtiff red clay. The bark itſelf is of a lighter red than that ſent out here to the hoſpital under the name of *red bark*. It inclines more to the colour of cinnamon. The bitterneſs and aſtringency appear to be greater than in either of the other barks.

I apprehend, the proper ſeaſon for obtaining it is about the month of March, before the flowers come out: after-experience will beſt determine this.

Infuſed in cold water, in which form, or in lime-water, I generally uſe it, it forms a very red tincture, poſſeſſing the

bitterneſs and aſtringency of the bark very ſtrongly. A few drops of the Tinctura florum martialium give it a very black colour, and occaſion a copious depoſition of a black ſediment. It does the ſame with the ſpirituous tincture.

With ſpirits it forms a beautiful red tincture.

Explanation of the references tab. XIX.

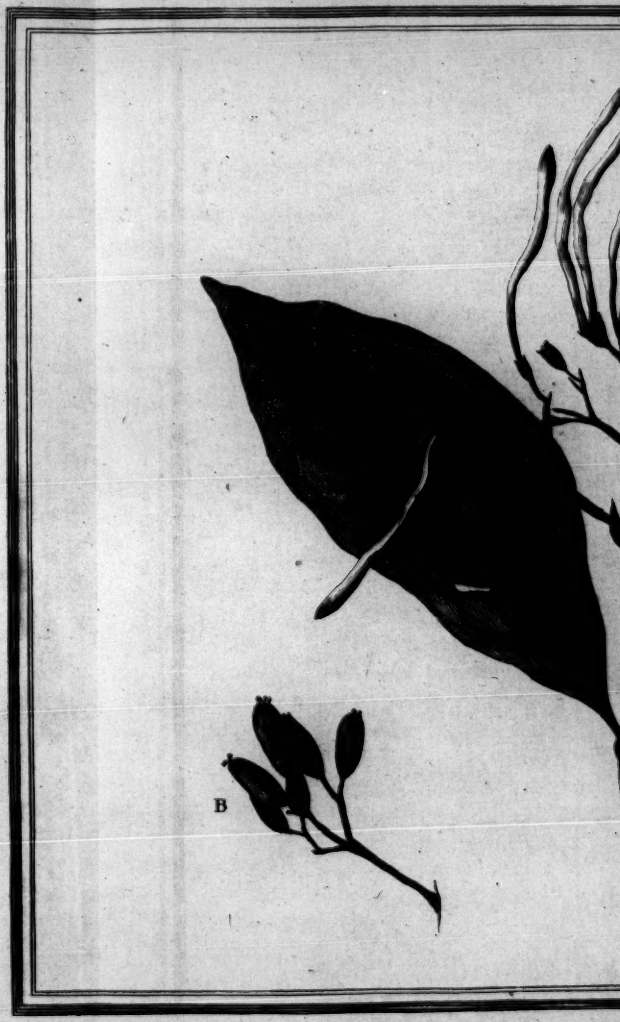
A. A branch of the Cinchona of St Lucia, with the flowers not yet opened.

B. The entire ſeed-veſſels.

C. A ſeed-veſſel ſplit.

D. One of the ſeeds, of its natural ſize.

E. The ſame magnified.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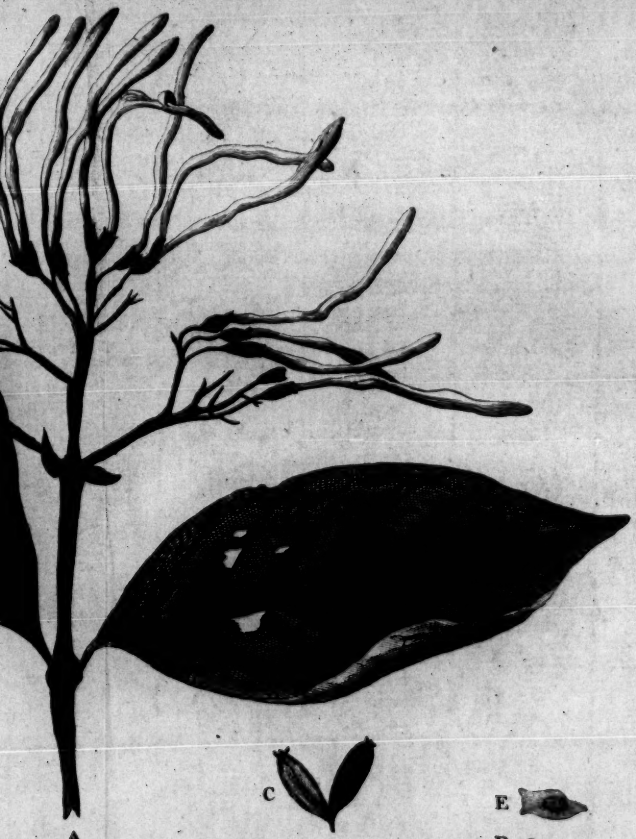
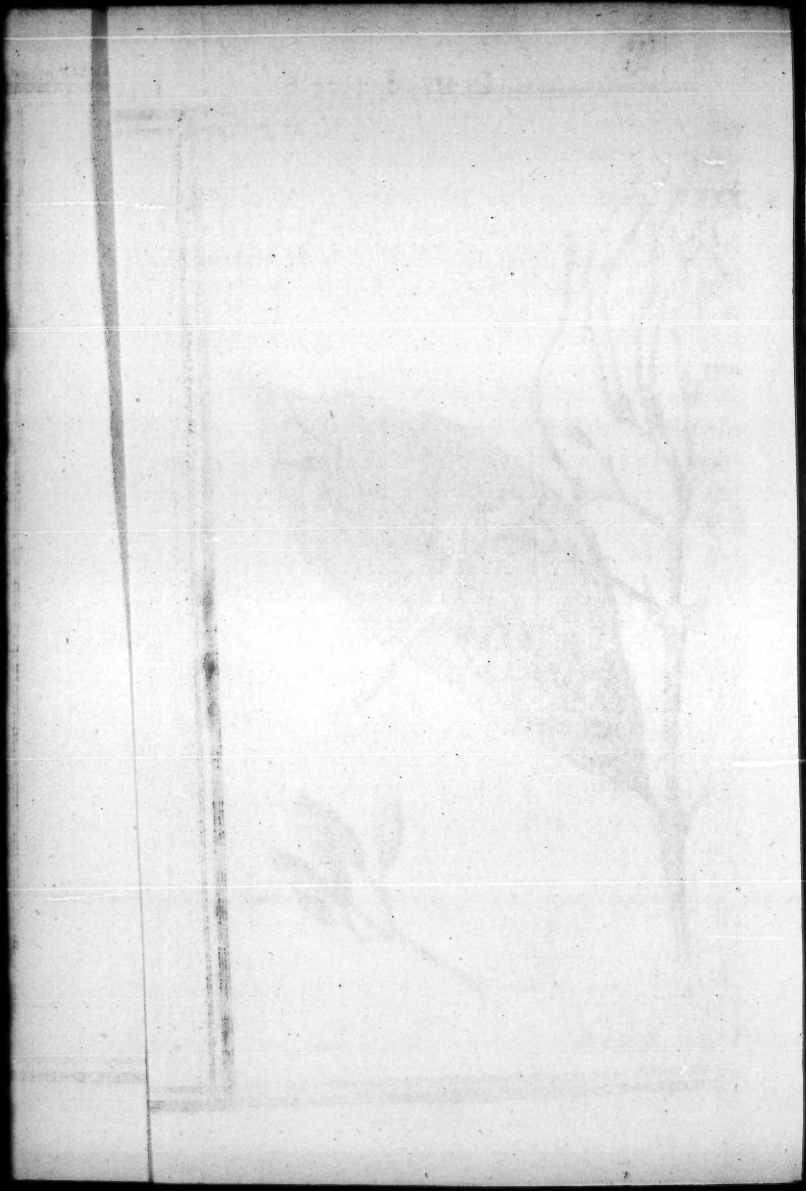


Rasires.



XXV. *An Account of an Observation of the Meteor of* August 18, 1783, *made on* Hewit Common *near* York. *In a Letter from* Nathaniel Pigott, *Esq. F. R. S. to the Reverend* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Read June 24, 1784.

REVEREND SIR, York, Oct. 18, 1783.

ON the 19th of last August I communicated to you an account of the remarkably fine meteor, which I had seen under circumstances peculiarly favourable the preceding night. I was then preparing myself for a journey into the East Riding; and, on that account, obliged to postpone the verifications, mentioned hereafter, till my return.

On the 18th of August, about ten o'clock P.M. after a hot day, the weather a little hazy, but not so as to obliterate the stars, and no wind, being on horseback, in company with two other gentlemen, on Hewit Common, about three miles from York, my attention was attracted towards the W.N.W. by several faint flashes of lightning, such as are often seen near the horizon, or which may be still better compared to flashes of an aurora borealis. Soon after which I perceived some luminous matter in motion, and collecting together from several directions, fig. 1. (tab. XX.) which immediately taking fire presented itself under the form of a ball, of so vivid a brightness, that the whole horizon was illuminated, so that the smallest object might

have been ſeen on the ground. This ball, when formed, began to move, with an eaſy ſliding motion, from W.N.W. towards the S.S.E. It ſuggeſted the idea of a highly brilliant comet, emitting a train or tail, but of a different colour from the ball itſelf, this laſt being of a moſt brilliant bluiſh white, and the tail of a duſky red, the length of which appeared to extend over fifteen or more degrees of the heavens, fig. 2. The apparent diameter of the nucleus ſeemed one-third or one-fourth of the full moon's diameter. The greateſt difficulty in this eſtimation hence ariſes, that I cannot, notwithſtanding all my endeavours, repreſent in my mind the moon otherwiſe than as a plane or diſk; nor the meteor, than as a ſpherical body. The altitude of it, when it formed in the W.N.W. was about 30°; and about 19° or 20° above the horizon, when it became extinct in the S.S.E. a few ſparks of the tail, neareſt the nucleus, ſcattering themſelves much in the ſame manner as thoſe of a ſky-rocket when burnt out, fig. 3.

It has been ſaid, that the ball divided itſelf into three or four parts before its extinction. To me it appeared to vaniſh or gently die away: what confirms me in the opinion, that it did not divide, is, that the three or four ſcattering parts above-mentioned were not of the bright colour of the ball itſelf, but of the duſky red which the tail invariably ſhewed. The interval of time from the meteor's formation to its extinction was nearly twenty ſeconds, perhaps two or three ſeconds leſs. The long habit I have of counting ſeconds in aſtronomical obſervations induces me to think this quantity may be relied on; and this I mention, becauſe ſome have eſtimated it more, ſome leſs. Nine or ten minutes after its diſſipation, I heard a noiſe, much reſembling the report of a cannon at a very great diſtance; but I would not wiſh to have it underſtood, that I

ſpeak

ſpeak to this laſt interval with the ſame certainty as to the other; if, however, it be exact, and ſuppoſing ſound to move 1106 feet in one ſecond of time, and the ſame in the upper regions of the atmoſphere as here below, which, however, may be very different, its diſtance from me, at its extinction, muſt have been about 120 miles, and its perpendicular altitude above the earth's ſurface about 40 miles.

I have added a ſcheme and a ſmall ſketch, preſuming by that means to convey a clearer idea of what I ſaw. The altitudes, azimuths, &c. are not merely from eſtimation. After my return from the Eaſt Riding, I went to the very ſpot, where I had ſeen the meteor on the 18th of Auguſt. The road, as in the ſcheme, being exactly ſtraight from my ſtation, both *towards* and *from* York, no miſtake can ariſe in that reſpect. With all the circumſtances clearly and forcibly impreſſed on my mind, I watched till ſome remarkable ſpot in the ſky preſented itſelf at the ſame place in which I had ſeen the meteor itſelf form, croſs the road, vaniſh, &c.: then, with a theodolite, I took the ſeveral bearings, which may be the more relied on, as I repeated the operations three different times, on different ſpots, which agree ſurpriſingly well for meaſures where no minute exactneſs can be expected. I have marked minutes in the ſcheme, becauſe the reſults gave them, without any pretenſion to ſuch nicety.

I am, &c.

NATH. PIGOTT.

XXXVI. *Observations of the Comet of* 1783. *In a Letter from* Edward Pigott, *Esq. to the Rev.* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Read June 24, 1784.

REVEREND SIR, York, Dec 11, 1783.

HAVING compleated my observations of the comet I discovered on the 19th of November last, I take the liberty of desiring you to present them to the Royal Society. The faintness of the comet's light, and the unfavourable sky you have had in the south, induce me to believe, that few observations of it have been made besides the following.

Dates.	Apparent time.	R. A.	North declination.	Greatest error of each R. A.	Longitude.	Latitude.
1783	h. ′	° ′ ″	° ′ ″	′ ″	S. ° ′	° ′
Novem. 19	11 4	41 1 30	3 11 $\frac{1}{2}$	3 00	1 9 37	12 $4\frac{1}{4}$ S
20	10 55 —	40 0 3	4 32 $\frac{1}{2}$	0 22	1 9 $2\frac{1}{3}$	10 31
22	6 52 +	38 21 10	6 50	0 30	1 8 11	7 50
24	10 24 —	36 29 28	9 36 $\frac{1}{4}$	0 15	1 7 19	4 $52\frac{1}{4}$
26	10 9 —	34 49 20	12 3 $\frac{1}{4}$	0 15	1 6 $33\frac{1}{7}$	2 6
Decem. 3	15 54	29 21 50	20 15 $\frac{1}{4}$	0 40	1 4 24	7 $42\frac{1}{2}$ N

The R. A's of November 20th, 24th, and 26th, were deduced from observations made at the transit instrument: the others, except the first, were determined with an excellent $2\frac{1}{2}$ feet night-glass, made by DOLLOND, magnifying 20 times, having cross wires at right angles in its focus, which were

Fig. 2.

Fig. 3.

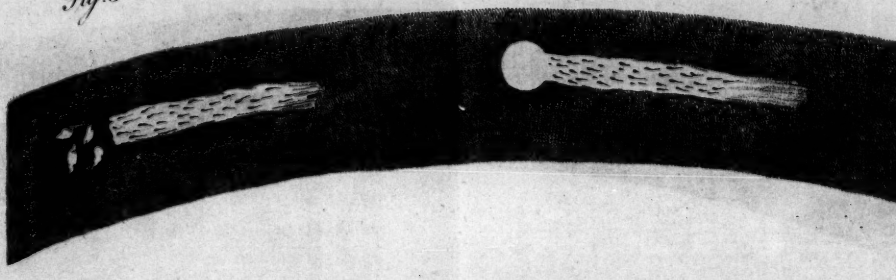
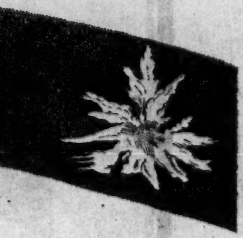
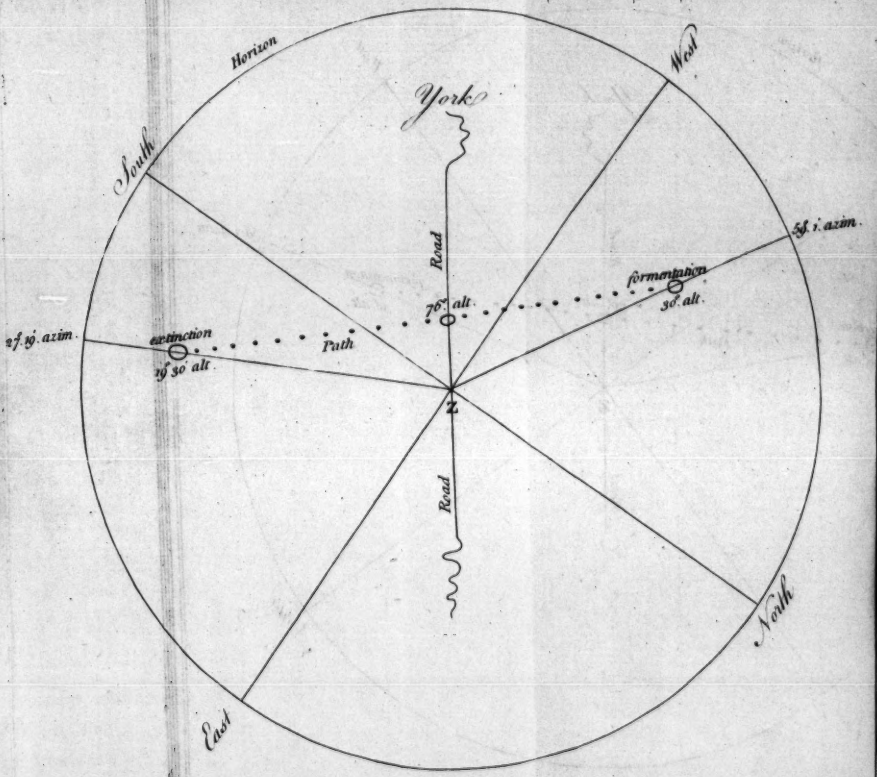


Fig. 1.





Basire Sc.

viſible without being illuminated. With this inſtrument the comet, by the common method, was compared to ſtars in the field of the teleſcope, and within four minutes on the ſame parallel. The places of thoſe ſtars were afterwards ſettled with the meridian inſtruments. As ſometimes ſeveral ſtars were obſerved, I eaſily found to what degree of certainty thoſe obſervations might be depended on, which I have marked with the above reſults. The declinations, I think, cannot err two minutes, being compared to ſtars within four minutes on the ſame parallel. The three of November 20th, 24th, and 26th, were taken with the tranſit inſtrument by comparing the comet to the neareſt ſtars. I was much chagrined in not being able to ſee the comet in our equatorial when the wires were illuminated.

The comet had exactly the appearance of a nebula: its light was ſo faint that it could not be ſeen in a good opera glaſs. In the night-teleſcope the nucleus was ſcarcely viſible, and the diameter of the ſurrounding coma was about three minutes of a degree. Between the 19th and 26th of November, I thought it had rather diminiſhed in brightneſs. December the 1ſt and 3d it was very difficult to be ſeen, occaſioned perhaps by its little elevation above the horizon. Between December the 3d and 10th, the comet was entirely effaced by the increaſed light of the Moon. On the 10th, the moon being in the horizon did not obliterate ſtars of the eighth or ninth magnitude; but I could not find the comet. The following obſervations were made by my friend Mr. JOHN GOODRICKE.

Dates.	Apparent time.	R. A.	North declination.	Longitude.	Latitude.
1783	h. ′	° ′ ″	° ′	S. ° ′	° ′
Novem. 24	8 16	36 32 57	9 30¾	1 7 21	1 42¼S
28	6 8¼	33 20 0	14 16¼	1 5 55½	0 52¼N

I am, &c.

EDW. PIGOTT.

P. S. This morning I received a letter from M. DE MECHAIN, in which he informs me, that he diſcovered the comet on the 26th of November ſeven days after my firſt obſervation. He has made ſeveral obſervations on it.

XXXVII. *Experiments on mixing Gold with Tin. In a Letter from Mr.* Staneſby Alchorne, *of his Majeſty's Mint, to* Peter Woulfe, *Eſq. F. R. S.*

Read June 24, 1784.

DEAR SIR,

Tower of London, March 25, 1784.

YOU know it is a generally received opinion among metallurgiſts, that tin has a property of deſtroying the ductility of gold, on being melted with it, even in very ſmall quantities. Our late ingenious countryman Dr. LEWIS, in his Philoſophical Commerce of Arts, p. 85. has well expreſſed the ſenſe of moſt writers on this ſubject, in the following words: "The moſt minute proportion of tin and lead," ſays he, "and even the vapours which riſe from them in the fire, "though not ſufficient to add to the gold any weight ſenſible "in the tendereſt balance, make it ſo brittle that it flies in "pieces under the hammer."

Divers circumſtances, nevertheleſs, long ſince induced me to diſbelieve the fact; but theſe, having chiefly ariſen from ſmall experiments, did not ſeem to warrant any general concluſion. A late public occaſion, however, which led me to various trials of mixing theſe metals together, in different proportions, and in ſufficiently large quantities, has put the matter out of diſpute; and ſhewn me, that tin, in ſmall quantity at leaſt, may be added to gold, either pure or alloyed, without producing any other effect than what might eaſily be conceived,

ceived, *à priori*, from the different texture of the two metals. In confirmation of which, I beg leave to lay ſome of the experiments before you.

EXPERIMENT I.

Sixty Troy grains of pure tin were ſtirred into twelve ounces of refined gold, in fuſion; and the mixture was then caſt into a mould of ſand, producing a flat bar, one inch wide, and one-eighth of an inch thick. The bar appeared ſound and good, ſuffered flatting under the hammer, drawing ſeveral times between a large pair of ſteel rollers, and cutting into circular pieces, of near an inch diameter, which bore ſtamping in the money-preſs, by the uſual ſtroke, without ſhewing the leaſt ſign of brittleneſs; or rather with much the ſame ductility as pure gold.

EXPERIMENT II.

Ninety grains of like tin were added to twelve ounces of fine gold, ſtirred, and caſt as above. The bar produced was ſcarcely diſtinguiſhable from the former, and bore all the operations, as before-mentioned, quite as well.

EXPERIMENT III.

One hundred and twenty grains of fine tin were mixed with twelve ounces of fine gold, and being caſt like the foregoing, produced a bar rather paler and harder than the preceding, but which ſuffered the like operations very well; except that, on drawing between rollers, the outer edges were diſpoſed to crack a little.

EXPE-

EXPERIMENT IV.

One hundred and forty grains, or half an ounce, of the like grained tin, were mixed, as before, with twelve ounces of fine gold; and the bar reſulting from this mixture was completely ſound and good; evidently paler and harder, however, than any of the foregoing, and cracking rather more than the laſt on paſſing between the rollers; but bearing every other operation, even ſtamping under the preſs, by the common force, without any apparent injury.

EXPERIMENT V.

One ounce of tin was next ſtirred into twelve ounces of the like refined gold, and then caſt as before; but the bar produced, though ſeemingly ſolid and good, was bad coloured, brittle in texture, and, on the firſt paſſing between the rollers, ſplit into ſeveral pieces, ſo that no farther trials were made with it.

EXPERIMENT VI.

To inquire how far the fumes of tin, brought into contact with the gold, would do more than mixing the metal in ſubſtance, a ſmall crucible, filled with twelve ounces of ſtandard gold, $\frac{11}{12}$ fine, was placed in a larger crucible, having one ounce of melted tin in it, and kept there in fuſion, the whole being covered by another large inverted crucible, for about half an hour. In this time a full quarter part of the tin was calcined; but the gold remained unaltered, and equally capable of being manufactured as another portion of the ſame gold melted in the common manner.

It

It may well be aſked, whether the tin, or part of it, in every trial, might not be deſtroyed, and thus render the concluſions fallacious? But as, in any of theſe experiments, not more than ſix or eight grains of the original weight were miſſing after the caſting, and as even fine gold can ſcarcely be melted without ſome loſs in the operation, ſo we may reaſonably ſuppoſe, that our ſmall loſſes, in the foregoing trials, do not deſerve conſideration.

The above experiments then ſeem to ſhew, that tin is not ſo miſchievous to gold as hath been generally repreſented. But it would be unfair to infer, that the original author of this doctrine (from whom ſo many have implicitly tranſcribed) had no foundation for the aſſertion. Gold and Tin, indeed, are ſubſtances pretty well known; but it is eaſy to imagine, that coins or trinkets may have been uſed for one, and impure tin, or pewter, perhaps, for the other; and it is difficult to gueſs what might be the reſult of ſuch uncertain combinations. To inquire farther, therefore, the experiments were continued as follows.

EXPERIMENT VII.

To determine whether the two metals might be more intimately combined, and the maſs rendered brittle, by additional heat; the mixture of gold and tin, produced in the firſt of theſe experiments, was re-melted in a ſtronger fire than before, and thus kept in fuſion full half an hour. By this operation ſix grains only were loſt in the weight; and the bar obtained was no leſs manufacturable than at firſt.

EXPE-

EXPERIMENTS VIII. AND IX.

The mixtures of gold and tin, from the ſecond and fourth experiments, were re-melted ſeparately, and one ounce of copper added to each. Being both well ſtirred, they were caſt as uſual; and the bars, though ſenſibly harder, bore all the operations of manufacturing as before. The laſt bar cracked a little at the edges, on drawing through the rollers, as it had done without the copper, but not materially, and bore cutting rather better than in its former ſtate.

EXPERIMENTS X. AND XI.

A quarter of an ounce of the laſt mixture (being tin half an ounce, and copper one ounce, with gold twelve ounces), and as much of the bar from experiment the third (being tin one hundred and twenty grains with gold twelve ounces), were each melted by a Jeweller, in the moſt ordinary manner, with a common ſea-coal fire, into ſmall buttons, without any loſs of weight. Theſe buttons were forged by him into ſmall bars, nealing them often by the flame of a lamp, and afterwards drawn each about twenty times through the apertures of a ſteel plate, into fine wire, with as much eaſe as coarſe gold commonly paſſes the like operation.

EXPERIMENT XII.

To enquire whether the adding of tin to gold, already alloyed, would cauſe any difference, ſixty grains of tin were ſtirred into twelve ounces of ſtandard gold, $\frac{11}{12}$ fine; and the reſult paſſed every operation before deſcribed, without ſhewing the leaſt alteration from the tin.

For greater certainty, ſeveral other trials were made, of different mixtures of copper, tin, and ſilver, with gold, even ſo

low as two ounces and a half of copper, with half an ounce of tin, to twelve ounces of gold. But these are not worth particularizing; for they all bore hammering, and flatting by rollers, to the thinness of stiff paper, and afterwards working into watch-cases, cane-heads, &c. with great ease. They all, indeed, grew more hard and harsh, in proportion to the quantity of alloy; but not one of them had the appearance of what all workmen well know by the name of brittle gold. Whence it should seem, that neither tin in substance, or the fumes of it, tend much to render gold unmanufacturable.

Whenever, therefore, brittleness has followed the adding small quantities of tin to fine gold, it must be supposed to have arisen from some unfriendly mixture in the tin, probably from Arsenic; for other experiments have shewn me, that twelve grains of regulus of arsenic, injected into as many ounces of fine gold, will render it totally unmalleable.

From the foregoing experiments, I presume, we may fairly conclude, that though tin, like other inferior metals, will contaminate gold, in proportion to the quantity mixed with it, yet there does not appear any thing in it specifically inimical to this precious metal. And this being contrary to the doctrine of most chemical writers, I submit to your better judgement, whether it may not be useful to publish these experiments, by laying them before the Royal Society.

I am, &c.

S. ALCHORNE.

XXXVIII. *Sur un moyen de donner la Direction aux Machines Aéroſtatiques. Par M. Le Comte* De Galvez. *Communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read July 1, 1784.

NOUS ſouſſignés certifions, que M. le Comte de Galvez nous ayant communiqué ſes idées ſur le moyen de pouvoir donner la direction aux machines aëroſtatiques, pour faire route à volonté et par un rumb certain et aſſuré dans l'air, principalement fondé ſur différentes obſervations qu'il avoit faites ſur l'uſage que les oiſeaux font de leurs aîles quand ils volent, et ſur celui que font les poiſſons de leurs nageoires et de leur queue quand ils nagent :

Nous nous ſommes tranſportés, l'apres-midi du prémier Mars de cette année 1784, au canal de Manzanarês, où on avoit preparée une chaloupe de vingt-cinq pieds de long ſur quatre et demi de large, avec une machine qu'il avoit inventée pour démontrer ſes idées. Cette machine *, qui conſiſtoit en un chevalet qui alloit de poupe à proue à la hauteur de cinq pieds, étoit croiſée en rectangles par trois vergues de bois élaſtique, de dix-huits pieds de long chacune, avec une aîle à chaque bout, compoſée de baguettes de baleine, couvertes d'un morceau de taffetas de cinq pieds de long, et trois de large, laquelle étoit jointe par un de ſes quatre côtés à la vergue, de façon que l'aîle reſtoit horiſontale. Le mouvement ſe communiquoit à chaque vergue, et par conſéquent à ſes deux aîles, par un ſeul homme, qui tirant avec viteſſe des cordes attachées aux bouts de chaque vergue, les agitoit verticalement, d'où reſultoit que

* See tab. XXI. fig. 1.

quand elles ſe plioient, les aîles prenoient à leurs extrémités une inclination de quarante-cinq degrés de l'horiſon. Ce mouvement et celui de la réaction produiſoient dans la chaloupe, où il y avoit ſix hommes, une impulſion qui la faiſoit marcher contre le courant du canal et le peu d'air qu'il faiſoit, cent cinquante pieds par minute, outre ſoixante pieds qu'elle parcouroit avant de s'arrêter depuis l'inſtant qu'on ceſſoit de mouvoir les aîles: elle parcouroit deux cents quarante-trois pieds par minute, allant avec le courant et l'air, par le même mouvement continu des aîles.

Nous fûmes tous très-etonnés de l'effet que produiſit cette expérience; car, quoique le deſir qu'avoit l'inventeur de mettre ſes idées en pratique au plutôt, fut cauſe qu'il ſe ſervit d'une chaloupe lourde et mal conſtruite, avec laquelle les aîles n'avoient point de proportion; nous ſommes perſuadés que la ſituation des aîles et leur mouvement vertical, qui formoient lors qu'on les battoient un plan incliné, imitant en cela les oiſeaux et les poiſſons, fourniſſent un principe ſûr et certain pour donner une direction par quelque rumb que ſe ſoit, à toute eſpèce de corps qui nagent dans un fluide, et par conſéquent très-applicable aux nouvelles machines aëroſtatiques.

Cette invention nous paroît digne de l'approbation et de l'eloge des phyſiciens qui, ſans doute, employeront leurs efforts pour lui donner toute la perfection dont elle eſt ſuſceptible dans l'exécution de ſon mécaniſme.

Et pour conſtater que la dite expérience a été faite de la manière qu'on vient d'expoſer, nous avons ſigné la préſente certification, ainſi qu'un deſſein de la dite machine, à Madrid le deux Mars, mil ſept cent quatre-vingt-quatre. D. JOSEF DE VIEXA, D. AGUSTIN BETANCOURT Y MOLINA, D. RICARDO WORSLEY, RAIM DE S. LAURENT, CASIMIRE ORTEGA.

XXXIX. *An extraordinary Caſe of a Dropſy of the Ovarium, with ſome Remarks. By Mr.* Philip Meadows Martineau, *Surgeon to the* Norfolk *and* Norwich *Hoſpital; communicated by* John Hunter, *Eſq. F. R. S.*

Read July 1, 1784.

SARAH KIPPUS, a pauper in the city of Norwich, was, for many years, a patient of my father's, and, at his deceaſe, was under the care of Mr. SCOTT, as city ſurgeon, who obliged me many times by taking me to the poor woman, from whom I received the account of the early part of her diſeaſe.

Her complaints came on firſt after a miſcarriage at the age of 27. She had never been pregnant before; and her diſcharges at that time were ſo great as to bring her into a very weak condition. She ſoon perceived ſome uneaſineſs, attended with a ſwelling, on one ſide, which, after a few months, became too large to diſtinguiſh whether it was greater on one ſide or the other. As the ſwelling was found to ariſe from water, it was drawn off, which was in the year 1757. She was never afterwards pregnant; but the catamenia continued regularly till the uſual period of their ceſſation. When I firſt ſaw her, which was in the year 1780, ſhe had been many times tapped, and ſhe was then full of water. Her appearance was truly deplorable, not to ſay ſhocking. She was rather a low woman, and her body ſo large as almoſt wholly to obſcure her face, as well as every other part of her: with all ſhe was tole-

rably chearful, and ſeldom regarded the operation. I ſaw her juſt before we took away 106 pints of water, and I begged leave to take a meaſure of her. She was ſixty-ſeven inches and a half in circumference, and from the cartilago enſiformis to the os pubis thirty-four inches. Her legs were now greatly ſwelled; but this, and every other ſymptom of which ſhe complained, evidently aroſe from the quantity and weight of water. She neither ate nor drank much, and made but a ſmall quantity of urine.

The operation of drawing off the water was generally performed on a Sunday, as the moſt convenient day for her neighbours to aſſiſt her, and before the latter end of the week ſhe was able to walk very well. She was firſt tapped in the year 1757, and died in Auguſt 1783. Thus ſhe lived full twenty-five years with ſome intervals of eaſe, having eighty times undergone the operation, and in all had taken from her 6631 pints of water, or upwards of thirteen hogſheads.

I will ſubjoin the account of the dates, and the quantity drawn off at each time, as given me by Mr. SCOTT, obſerving that till 1769 no exact memorandum was kept, except of the *number* of times, although the quantity of water drawn off was always meaſured. By my father ſhe was tapped twenty-ſix times, averaged at 70 pints each time: by Mr. DONNE once, 73 pints, which makes 1683 pints from ſome part of the year 1757 to 1769. By Mr. SCOTT as follows:

1769.

1769.	Pints.
Mar. 16.	70
July 17.	72
Nov. 20.	78
Dec. 31.	70
	290
1770.	
April 15.	70
Aug. 11.	73
Dec. 4.	76
	219
1771.	
Mar. 22.	74
July 14.	78
Nov. 3.	79
	231
1772.	
Feb. 22.	79
June 6.	73
Sept. 12.	74
Dec. 12.	82
	308
1773.	
March 7.	78
May 29.	71
Aug. 29.	79
Dec. 5.	81
	309

1774.	Pints.
Mar. 13.	77
June 26.	89
Oct. 23.	92
	258
1775.	
Jan. 24.	94
May 28.	91
Sept. 13.	72
Dec. 16.	80
	337
1776.	
April 9.	84
July 28.	82
Nov. 27.	85
	251
1777.	
Mar. 16.	89
July 27.	90
Nov. 9.	98
	277
1778.	
March 8.	96
July 5.	99
Nov. 5.	105
	300

1779.	Pints.
Feb. 28.	106
June 13.	108
Aug. 17.	92
Oct. 24.	99
Dec. 10.	90
	495
1780.	
Feb. 6.	73
Apr. 23.	102
July 24.	106
Sept. 10.	95
Nov. 12.	98
	474
1781.	
Jan. 1.	100
Mar. 11.	94
June 25.	100
Oct. 14.	100
	394
1782.	
Jan. 13.	99
Mar. 18.	64
June 2.	74
Aug. 25.	98
Nov. 17.	90
	425

1783.	Pints.
Feb. 14.	104
May 11.	100
July 20.	98
Aug. 11 on opening	78
	380
Total	6631 pints.

In looking over this account it appears, that 108 pints was the greateſt quantity ever taken away at any one time; that ſhe was never tapped more than five times in any one year; and the largeſt quantity in a year was 495 pints. The moſt collected in the ſhorteſt ſpace of time was 95 pints in ſeven weeks, from July 24th to September 10th in 1780, which is very nearly two pints a day. It appears alſo, that in the laſt 14 years of her life, when a regular account was kept, ſhe increaſed faſter in the winter than in the ſummer months. If the ſix ſummer months from April to September incluſive are reckoned, ſhe loſt in the 14 years in 23 operations 1972 pints, and in the winter months from October to March incluſive, by 30 tappings, 2596 pints; and it will be found, that 30 is to 2596 rather more than 23 to 1972, ſo that ſeven more tappings were at leaſt neceſſary in the winter than in the ſummer. In the months of March and November ſhe oftener underwent the operation than in any other. In theſe calculations the three months in 1783 are not included, as the year was not finiſhed.

If we compare the famous caſe of Lady PAGE, related by Dr. MEAD, the quantity of water taken from her ladyſhip appears ſmall when oppoſed to the number of pints drawn from

from SARAH KIPPUS. The one loſt 1920, the other 6631. It muſt be confeſſed, however, that Lady PAGE collected faſter than the poor woman whoſe caſe I have related.

I come now to ſpeak of the diſſection, and to make ſome obſervations on the whole. On the 10th of Auguſt 1783, the poor woman died; and the following day Dr. DACK, an eminent phyſician of this place, accompanied me to open the body. I firſt drew off 78 pints of clear water: ſuppoſing, therefore, all the water to have been taken away at the laſt operation, then in three weeks ſhe had collected 78 pints, which is more than three pints and a half in each day: a quantity far exceeding what ſhe had taken. I then opened into the cavity from which the water came, and ſeparated the ſac from the peritoneum, and found the ſac had ariſen in the ovarium of the left ſide. After this, I diſſected out the uterus, with the right ovarium in a natural ſtate, and thus obtained every part neceſſary to ſhow the diſeaſe, *viz.* the uterus, the right ovarium ſound, and the left enlarged into an immenſe pouch. The cyſt itſelf was not very thick, but lined in almoſt every part of it, but more eſpecially in the fore part, with ſmall oſſifications. The peritoneum was *prodigiouſly* thickened, and thus, by its additional ſtrength, became the chief ſupport of the water. There was ſomething ſingular in the ſac itſelf, for it was rather two than one, from there being an opening in the ſide of what appeared at firſt the only cavity, which led to another cavity, almoſt equally large with the firſt, ſo that if all the water in any operation had not been evacuated, it muſt probably have been owing to a difficulty in its paſſage from the ſecond into the firſt or more external cyſt. From the ſize, however, of the poor woman after each operation, it is evident, that in her there being two ſacs did not prevent the total drawing off of

the water. The other viſcera appeared all in a natural ſtate. The inteſtines were quite empty, and puſhed up under the ribs, ſo as to have left but very little room for the expanſion of the lungs within the thorax. The bladder was contracted, or rather I ſhould ſay appeared leſſened. The kidneys were healthy, and both ureters in a natural ſtate. The ſac is in the collection of JOHN HUNTER, eſq.

In reflecting upon this caſe, an obvious queſtion ariſes; from whence proceeded this immenſe collection of water? At different periods of this poor woman's life the quantity drawn off, without conſidering the urine ſhe made, was much greater than the fluids ſhe drank, which appeared from meaſuring whatever ſhe took. It appears then pretty certain, that this ſuperabundant quantity muſt have been taken into the body by abſorption; and if we allow the bodies of animals to have this power of abſorbing, which we very well know vegetables are poſſeſſed of, it will account for many appearances in the animal œconomy. This poor woman collected faſter in the wet moiſt months of winter, than in ſummer.

From all, this happy concluſion may be drawn, that although human art is at preſent inſufficient to the perfect cure of diſeaſes ſimilar to the poor woman's caſe I have related, yet nature is continually defending herſelf from ſudden death; and ſuch relief *may* be granted as to protract life a long time without much pain, and often with intervals of great eaſe and comfort.

XL. *Methodus inveniendi Lineas Curvas ex proprietatibus Variationis Curvaturæ.* *Auctore* Nicolao Landerbeck, *Matheſ. Profeſſ. in Acad. Upſalienſi Adjuncto. Communicated by* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read July 1, 1784.

PARS SECUNDA*.

CURVAS, ex proprietate variationis curvaturæ invenire, indice per functionem coordinatarum cujuſdam expreſſo, problema etſi indeterminatum eſt; juvat tamen ad curvas cognoſcendas, quum facile et ſponte ſeſe offerunt conditiones determinantes qui rei conveniunt et quæ in caſu quovis examini ſubjecto locum habent. Quo conſilio et qua arte calculum inire oporteat, ut et hæc et his affinia peragenda ſint, quæ ad curvas ex curvaturæ variatione cognoſcendas pertineant, per theoremata quæ ſequuntur, exponere conabor.

THEOREMA I. (Vide tab. XXI. fig. 2.)

Si curvæ cujuſdam LC index variationis curvaturæ ſit T, radius curvedinis R, ſinus anguli BCD p, poſito ſinu toto 1, arcus curvæ LC z coordinatæ perpendiculares x et y earumque fluxiones dp, dz, dx, et dy dicantur, erit $\frac{dz}{\int T dz} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam $dx = -R dp$ et $dz = -\frac{dx}{\sqrt{1-p^2}}$ habetur $\frac{dz}{R} = -\frac{dp}{\sqrt{1-p^2}}$

* See Vol. LXXIII. p. 456.

et quum $dR = T dz$ erit $R = \int T dz$ et substitutione $\frac{dz}{\int T dz} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 1. Hinc obtinetur $\frac{dx}{R} = -dp$, $\frac{dy}{R} = -\frac{p\,dp}{\sqrt{1-p^2}}$ et $\frac{dz}{R} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 2. Si Tangens anguli BCD per r, Secans per s designentur habetur $\frac{dz}{\int T dz} = -\frac{dr}{1+r^2}$ et $\frac{dz}{\int T dz} = -\frac{ds}{s\sqrt{s^2-1}}$.

Schol. 1. Ex hoc theoremate facilis deducitur methodus generaliter calculandi variationem curvaturæ curvæ cujuscumque. Nam $\int T dz = -\frac{dz\sqrt{1-p^2}}{dp}$, quantitas vero $\frac{dz\sqrt{1-p^2}}{dp}$ datur, data inter x et y relatione. Sit valor quantitatis $-\frac{dz\sqrt{1-p^2}}{dp} = Z$ functioni curvæ z, $\int T dz = Z$ et sumtis fluxionibus $T dz = \dot{Z} dz$ qua $T = \dot{Z}$ functioni ipsius z. Si valor quantitatis $-\frac{dz\sqrt{1-p^2}}{dp} = P$ per p expressus, erit $\int T dz = P$ sumtisque fluxionibus $T dz = \dot{P} dp$ et $T = \frac{\dot{P} dp}{dz}$, quæ functio est quantitatis p, in potestate semper est $\frac{dp}{dz}$ per p exprimere.

Schol. 2. Hujus etiam theorematis subsidio inveniri possunt curvæ ex data relatione inter T et z, R et z, R et y, et R et p. Si enim sit $T = Z$ functioni quantitatis z, erit $\int T dz = \int Z dz + A$, vi theorematis $\frac{dz}{\int Z dz + A} \left(= \frac{dz}{\int T dz}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{dz}{\int Z dz + A} + C = -\frac{dp}{\sqrt{1-p^2}}$. Posita $\int \frac{dz}{\int Z dz + A} + C = b$ et N numerus

merus cujus logarithmus hyperbolicus 1 habetur $\sqrt{1-p^2} = \frac{N^{b\sqrt{-1}} - N^{-b\sqrt{-1}}}{2\sqrt{-1}}$ et $p = \frac{N^{b\sqrt{-1}} + N^{-b\sqrt{-1}}}{2}$, quæ functiones ſunt quantitatis z, quibus poſitis $\dot{Z}$ et $\sqrt{1-\dot{Z}^2}$ reſpective proveniunt $x\,(=\int dz\sqrt{1-p^2}) = \int Z dz$ et $y\,(=\int pdz) = \int dz\sqrt{1-\dot{Z}^2}$ quarum alterutra curvarum indoles innoteſcit.

Si $R = X$ functioni abſciſſæ x provenit $\frac{dx}{X}\,(=\frac{dx}{R}) = -dp$ et integratione $\dot{X}\,(=C-\int\frac{dx}{R}) = p$ unde $\sqrt{1-p^2} = \sqrt{1-\dot{X}^2}$ et $y\,(=\int\frac{pdx}{\sqrt{1-p^2}}) = \int\frac{\dot{X}dx}{\sqrt{1-X^2}}$ æquatio curvæ indolem exprimens.

Et ſi $R = Y$ functioni ordinatæ y, habetur $\frac{dy}{Y}\,(=\frac{dy}{R}) = -\frac{pdp}{\sqrt{1-p^2}}$ et integratione $\dot{Y}\,(=\int\frac{dy}{Y}+C) = \sqrt{1-p^2}$, unde $p = \sqrt{1-\dot{Y}^2}$ et $x\,(=\int\frac{dy\sqrt{1-p^2}}{p}) = \int\frac{\dot{Y}dy}{\sqrt{1-\dot{Y}^2}}$ quæ exprimit naturam curvæ.

Hinc colligitur quod quoties Tdz perfecte integretur et $\int\frac{dz}{\int Zdz+A}$ obtineatur per arcus circulares dum aut $\int\dot{Z}dz$ aut $\int dz\sqrt{1-\dot{Z}^2}$ abſolutam admittat integrationem, curvæ erunt rectificabiles, et algebraicæ, ſi relatio inter x et z vel inter y et z in relationem algebraicam inter x et y permutari poſſit.

Evidens etiam eſt quod ſi X functio eſt algebraica quantitatis x vel Y quantitatis y, et non ſolum $\frac{dx}{X}$ vel $\frac{dy}{Y}$ ſed etiam $\frac{\dot{X}dx}{\sqrt{1-\dot{X}^2}}$ vel $\frac{\dot{Y}dy}{\sqrt{1-\dot{Y}^2}}$ quantitates perfecte integrabiles, curvæ evadunt algebraicæ, alias tranſcendentes.

Exempl.

Exempl. 1. Invenienda sit curva ubi variatio curvaturæ $T = \frac{3 \cdot \overline{8a+27z}^{\frac{2}{3}} - 9a^{\frac{2}{3}}}{a^{\frac{1}{3}}\sqrt{\overline{8a+27z}^{\frac{2}{3}} - 4a^{\frac{2}{3}}}}$. Ut simplicior reddatur calculus ponatur $\overline{8a+27z}^{\frac{1}{3}} = u$ et $a^{\frac{1}{3}} = b$ erit $z = \frac{u^3 - 8b^3}{27}$, $dz = \frac{du\sqrt{u}}{18}$, $T = \frac{3u-2b}{\sqrt{b}\sqrt{u-4b}}$ et $\int T dz = \frac{u\sqrt{u}\sqrt{u-4b}}{18\sqrt{b}} + A$; sit constans hæc $A = a$, quod accidit evanescente $\int T dz u = b$, habetur per theorema $\frac{du\sqrt{b}}{u\sqrt{u-4b}} \left(= \frac{dz}{\int T dz}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{du\sqrt{b}}{u\sqrt{u-4b}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, cujus æquationis termini quum sint arcus circulares quorum sinus $\sqrt{1-p^2} = \frac{\sqrt{u-4b}}{\sqrt{u}}$ et cosinus $p = \frac{2\sqrt{b}}{\sqrt{u}}$, posito arcu constanti $C = 0$, obtinetur $y \left(= \int p dz\right) = \int \frac{du\sqrt{b}}{9} + B) = \frac{\sqrt{u-4b}\sqrt{b}}{9}$ nam $B = \frac{4b\sqrt{b}}{9}$, posita $y = 0$ et $u = 4b$, atque $x \left(= \int dz\sqrt{1-p^2}\right) = \int \frac{du\sqrt{u-4b}}{18} = \frac{\overline{u-4b}^{\frac{3}{2}}}{27}$ quibus æquationibus exterminata u et substituta a habetur $y^3 = ax^2$ æquatio pro parabola cubica.

Exempl. 2. Si sit variatio curvaturæ $T = \frac{2z}{a}$ erit $\int T dz \left(= \int \frac{2z dz}{a}\right) = \frac{z^2}{a} + A$ et si $Z = 0$ $\int T dz = a$ erit constans $A = a$, atque vi theorematis $\frac{a dz}{a^2+z^2} \left(= \frac{dz}{\int T dz}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int \frac{a dz}{a^2+z^2} + C = -\int \frac{dp}{\sqrt{1-p^2}}$; posito arcu constanti $C = 0$ cæteri sunt æquales eorumque sinus et cosinus, unde $\sqrt{1-p^2} = \frac{z}{\sqrt{a^2+z^2}}$, $p = \frac{a}{\sqrt{a^2+z^2}}$ et $dx \left(= dz\sqrt{1-p^2}\right) = \frac{z dz}{\sqrt{a^2+z^2}}$ et $dy \left(= p dz\right)$

$pdz) = \frac{adz}{\sqrt{a^2+z^2}}$, quibus conſtat curvam eſſe catenariam.

Exempl. 3. Sit variatio curvaturæ $T = \frac{a-z}{\sqrt{2az-z^2}}$, evadit $\int T dz = \sqrt{2az-z^2}$, per theorema $\frac{dz}{\sqrt{2az-z^2}} \left(= \frac{dz}{\int T dz}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et per integrationem $\int \frac{dz}{\sqrt{2az-z^2}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, ſi arcus ille conſtans $C = o$, cæteri ſunt æquales eorumque ſinus et coſinus, quo $\sqrt{1-p^2} = \frac{\sqrt{2az-z^2}}{a}$, $p = \frac{a-z}{a}$ et $y \left(= \int p dz\right) = \int \frac{a-z dz}{a}\Big) = \frac{2az-z^2}{a}$ æquatio pro cycloide ordinaria.

THEOREMA II.

Manentibus antea adhibitis denominationibus erit $\frac{dx}{y+\int T dx} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam $\frac{dx}{R} = -dp$, erit dividendo per $\sqrt{1-p^2}$, $\frac{dx}{R\sqrt{1-p^2}} = -\frac{dp}{\sqrt{1-p^2}}$. Propter $1 : \sqrt{1-p^2} :: CD(R) : CF = R\sqrt{1-p^2}$, ſed $dz : dx :: Tdz : Tdx$, quæ fluxio eſt ipſius DE, quare $DE = \int T dx$, unde $CF = y + \int T dx$ qua pro $R\sqrt{1-p^2}$ ſubſtituta, prodit $\frac{dx}{y+\int T dx} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. 1. Quantitas $dy + Tdx$ ſemper eſt perfecte integrabilis. Nam $Tdx = -\frac{ddx\sqrt{1-p^2}}{dp}$ et $dy = \frac{pdx}{\sqrt{1-p^2}}$ unde $dy + Tdx = \frac{pdx}{\sqrt{1-p^2}} - \frac{ddx\sqrt{1-p^2}}{dp}$ et integratione $y + \int T dx = -\frac{dx\sqrt{1-p^2}}{dp}$.

Cor.

Cor. 2. Dicatur ſemichorda curvaturæ CF F, obtinetur $\frac{dx}{F} = -\frac{dp}{\sqrt{1-p^2}}$, $\frac{dy}{F} = -\frac{pdp}{1-p^2}$ et $\frac{dz}{F} = -\frac{dp}{1-p^2}$.

Cor. 3. Si Tangens anguli BCD per r, Secans per s deſignentur habetur $\frac{dx}{y+\int T dx} = -\frac{dr}{1+r^2}$ et $\frac{dx}{y+\int T dx} = -\frac{ds}{s\sqrt{s^2-1}}$.

Schol. 1. Per hoc theorema via etiam patet calculandi generaliter variationem curvaturæ. Eſt enim $y+\int T dx = -\frac{dx\sqrt{1-p^2}}{dp}$, quantitas vero $\frac{dx\sqrt{1-p^2}}{dp}$ datur data inter x et p relatione. Sit valor quantitatis $-\frac{dx\sqrt{1-p^2}}{dp} = X$ functioni abſciſſæ x æquatione ad curvam inventus, erit $\int T dx = X - y$ et ſumtis fluxionibus $T dx = \dot{X} dx - dy$, qua $T = \dot{X} - \frac{dy}{dx}$ ubi tam $\dot{X}$ quam $\frac{dy}{dx}$ ſunt functiones abſciſſæ x. Si valor quantitatis $-\frac{dx\sqrt{1-p^2}}{dp} = P$ per p expreſſus, erit $\int T dx = P - y$ ſumtiſque fluxionibus $T dx = \dot{P} dp - dy$, qua $T = \frac{\dot{P} dp}{dx} - \frac{p}{\sqrt{1-p^2}}$ ubi $\frac{\dot{P} dp}{dx}$ functio eſt quantitatis p, nam $\frac{dp}{dx}$ per p exprimi poteſt.

Schol. 2. Hoc adhibito theoremate inveniri etiam poſſunt curvæ, ex data relatione inter T et x, F et x, F et y, F et z, et F et p. Poſita enim T functione quantitatis x, patet per curvarum quadraturas, aut perfectam aut imperfectam quantitatis $T dx$ obtineri integrationem. Sit $\int T dx = \dot{X} + \int \dot{X} dx$ functioni vel algebraicæ vel ex parte tranſcendenti ipſius x, cujus terminis homogeneus valor ipſius $y = \int \ddot{X} dx$ capiatur, iſque ejus indolis ut $\int \dot{X} + \ddot{X} dx$, vel quod idem eſt $y + \int T dx =$

$X + \int \sqrt{X + \overset{\text{II}}{X} dx}$ integratione abſoluta habeatur, permanente $T dz = T dx \sqrt{1 - \overset{\text{II}}{X}{}^2}$ perfecte integrabili. Per theorema deinde habetur $\frac{dx}{X + \int \sqrt{X + \overset{\text{II}}{X} dx}}$ $\left(= \frac{dx}{y + \int T dx}\right) = -\frac{dp}{\sqrt{1 - p^2}}$, et per integrationem $\int \frac{dx}{X + \int \sqrt{X + \overset{\text{II}}{X} dx}} + C = -\int \frac{dp}{\sqrt{1 - p^2}}$, ſi ponatur $\int \frac{dx}{X + \int \sqrt{X + \overset{\text{II}}{X} dx}} + C = k$ et N baſi logarithmorum hyperbolicorum, erit $\sqrt{1 - p^2} = \frac{N^{k\sqrt{-1}} - N^{-k\sqrt{-1}}}{2\sqrt{-1}}$ et $p = \frac{N^{k\sqrt{-1}} + N^{-k\sqrt{-1}}}{2}$, $\sqrt{1 - p^2}$ et p igitur ſunt functiones ipſius x, quæ ſi ponantur $\sqrt{1 - \overset{\text{II}}{X}{}^2}$ et $\overset{\text{III}}{X}$, habetur $y \left(= \int \frac{p dx}{\sqrt{1 - p^2}}\right) = \int \frac{\overset{\text{III}}{X} dx}{\sqrt{1 - \overset{\text{II}}{X}{}^2}}$, æquatio qua curvæ internoſcuntur.

Si ſit $F = Y$ functioni quantitatis y erit per Cor. 2. $\frac{dy}{Y}\left(= \frac{dy}{F}\right) = -\frac{dp}{1 - p^2}$ et integratione $\int \frac{dy}{Y} + \log. C = \log. \sqrt{1 - p^2}$, ponatur $\int \frac{dy}{Y} = k$ et N logarithmorum baſi, erit facto ad quantitates abſolutas tranſitu $CN^k = \sqrt{1 - p^2}$, $p = \sqrt{1 - C^2 N^{2k}}$ et $x \left(= \int \frac{dy \sqrt{1 - p^2}}{p}\right) = \int \frac{CN^k dy}{\sqrt{1 - C^2 N^{2k}}}$, æquatio quæ indolem curvæ indigitat.

Si $F = Z$ functioni ipſius z erit $\frac{dz}{Z}\left(= \frac{dz}{F}\right) = -\frac{dp}{1 - p^2}$ et integratione $\int \frac{dz}{Z} + \log. C = \log. \sqrt{\frac{1 - p}{1 + p}}$, et ſi $\int \frac{dz}{Z} = k$ et N baſi logarithmica habetur $p = \frac{1 - C^2 N^{2k}}{1 + C^2 N^{2k}}$ et $y = \int p dz\Big) = \int \frac{\sqrt{1 - C^2 N^{2k}}\, dz}{1 + C^2 N^{2k}}$ qua curvæ cognoſcuntur.

Conſtat hinc quod quoties $X+\int X dx$ perfecta integratione habeatur $\int \frac{dx}{X+\int X+X dx}$ per arcus circulares dum $\frac{\overset{\text{III}}{X}dx}{\sqrt{1-\overset{\text{III}}{X}^2}}$ abſolutam admittat integrationem curva ſit algebraica, ſi vero aliter evenerit tranſcendens.

Quoties $\frac{dy}{Y}$ ſit integrale logarithmicum et $\frac{CN^k dy}{\sqrt{1-CN^{2k}}}$ abſolutam admittat integrationem curva eſt algebraica, in aliis caſibus tranſcendens.

Et quoties $\int\frac{dz}{Z}$ per logarithmos inveniatur, $\frac{1-C^2N^{2k}dz}{1+C^2N^{2k}}$ abſolute ſit integrabilis pariter ac $\frac{2CN^k dz}{1+C^2N^{2k}}$ curva eſt algebraica, alias tranſcendens.

Exempl. 1. Si ſit variatio curvaturæ $T=\frac{3\cdot b^2-a^2x\sqrt{a^2-x^2}}{a^3b}$ erit $\int T dx\left(=\frac{a^3-b^2\cdot a^2-x^2\sqrt{a^2-x^2}}{a^3b}\right)=\frac{a\sqrt{a^2-x^2}}{ab}-\frac{x^2\sqrt{a^2-x^2}}{ab}-\frac{b\sqrt{a^2-x^2}}{a}+\frac{bx^2\sqrt{a^2-x^2}}{a^3}$, ſi ponatur $y=\frac{b\sqrt{a^2-x^2}}{a}$ habetur $y+\int T dx=\frac{a^4+b^2-a^2x^2\sqrt{a^2-x^2}}{a^3b}$, adhibendo theorema $\frac{a^3bdx}{a^4+b^2-a^2x^2\sqrt{a^2-x^2}}\left(=\frac{dx}{y+\int T dx}\right)=-\frac{dp}{\sqrt{1-p^2}}$ et integrando $\int\frac{a^3bdx}{a^4+b^2-a^2x^2\sqrt{a^2-x^2}}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, cujus termini ſunt arcus circulares quorum ſinus $\sqrt{1-p^2}=\frac{a\sqrt{a^2-x^2}}{\sqrt{a^4+b^2-a^2x^2}}$ et coſinus $p=\frac{bx}{\sqrt{a^4+b^2-a^2x^2}}$ evaneſcente arcu conſtanti C, quare $y\left(=\int\frac{pdx}{\sqrt{1-p^2}}\right)=\int\frac{bdx}{a\sqrt{a^2-x^2}}\Big)=\frac{b\sqrt{a^2-x^2}}{a}$ et in hoc caſu curva eſt ellipſis.

Exempl. 2. Sit jam variatio curvaturæ $T = \frac{2\sqrt{2ax+x^2}}{a}$ erit $\int T dx = \frac{x\sqrt{2ax+x^2}}{a} + \int \frac{x dx}{\sqrt{2ax+x^2}}$ et posita $y = \int \frac{a dx}{\sqrt{2ax+x^2}}$ perfecta integratione habetur $y + \int T dx = \frac{a+x\sqrt{2ax+x^2}}{a}$. Theorematis itaque auxilio erit $\frac{a dx}{a+x\sqrt{2ax+x^2}} \left(= \frac{dx}{y+\int T dx} = -\frac{dp}{\sqrt{1-p^2}}\right)$, et integratione $\int \frac{a dx}{a+x\sqrt{2ax+x^2}} = C = -\int \frac{dp}{\sqrt{1-p^2}}$, si vero arcus ille constans $C = 0$ cæteri sunt æquales eorumque sinus et cosinus, unde $\sqrt{1-p^2} = \frac{\sqrt{2ax+x^2}}{a+x}$, $p = \frac{a}{a+x}$ et $y \left(= \int \frac{p dx}{\sqrt{1-p^2}}\right) = \int \frac{a dx}{\sqrt{2ax+x^2}}$, æquatio indicans curvam esse catenariam.

THEOREMA III.

Dicatur cosinus anguli BCD q, posito radio 1, cæterisque manentibus denominationibus erit $\frac{dy}{\int T dy - x} = \frac{dq}{\sqrt{1-q^2}}$.

Est enim $\frac{dy}{R} = dq$, qua per $\sqrt{1-q^2}$ divisa, dat $\frac{dy}{R\sqrt{1-q^2}} = \frac{dq}{\sqrt{1-q^2}}$; et ob $1 : \sqrt{1-q^2} :: CD\,(R) : CG = R\sqrt{1-q^2}$, sed $dz : dy :: T dz : T dy$ cujus integrale est $AE = \int T dy$, unde $CG\,(= AE - AB) = \int T dy - x$, qua pro $R\sqrt{1-q^2}$ substituta, prodit $\frac{dy}{\int T dy - x} = \frac{dq}{\sqrt{1-q^2}}$.

Cor. 1. Semper $T dy - dx$ admittit perfectam integrationem. Etenim $T dy = \frac{ddy\sqrt{1-q^2}}{dq}$ et $dx = \frac{q dy}{\sqrt{1-q^2}}$, quibus $T dy - dx = \frac{ddy\sqrt{1-q^2}}{dq} - \frac{q dy}{\sqrt{1-q^2}}$ et integratione $\int T dy - x = \frac{dy\sqrt{1-q^2}}{dq}$.

Cor. 2. Dicatur ſemichorda curvaturæ CG G, habetur $\frac{dy}{G} = \frac{dq}{\sqrt{1-q^2}}$, $\frac{dx}{G} = \frac{q\,dq}{1-q^2}$ et $\frac{dz}{G} = \frac{dq}{1-q^2}$.

Cor. 3. Dicatur cotangens anguli BCD t, et cosecans v erit $\frac{dy}{\int T dy - x} = \frac{dt}{1+t^2}$ et $\frac{dy}{\int T dy - x} = \frac{dv}{v\sqrt{v^2-1}}$.

Schol. 1. Quoniam $\int T dy - x = \frac{dy\sqrt{1-q^2}}{dq}$ datur ex data relatione inter y et q, ſit $\frac{dy\sqrt{1-q^2}}{dq} = Y$ functioni ordinatæ y erit $\int T dy = Y - x$ ſumtiſque fluxionibus $T dy = \dot{Y} dy - dx$ qua $T = \dot{Y} - \frac{dx}{dy}$ functioni ipſius y. Si autem $\frac{dy\sqrt{1-q^2}}{dq} = Q$ functioni ipſius q erit $\int T dy = Q - x$ et ſumtis fluxionibus $T dy = \dot{Q} dq - dx$, qua habetur $T = \frac{\dot{Q} dq}{dy} - \frac{q}{\sqrt{1-q^2}}$ per q.

Schol. 2. Hujus theorematis auxilio elicere licet curvas data relatione inter T et y, G et y, G et x, G et z, et G et q. Si enim ſit T functio ipſius y generaliter $\int T dy = Y + \int \dot{Y} dy + A$, quæ functio eſt algebraica ipſius y quoties $\int \dot{Y} dy$ abſolute ſumi poſſit. Aſſumatur $x = \int \ddot{Y} dy$, tali ipſius y functioni ut non ſolum $\int T dy - x = Y + \int \dot{Y} + \ddot{Y} dy$ ſed etiam $\int T dz = \int T dy \sqrt{1 - \ddot{Y}^2}$ abſoluta integratione habeantur, provenit vi theorematis $\frac{dy}{Y + \int \dot{Y} + \ddot{Y} dy + A} \left(= \frac{dy}{\int T dy - x} \right) = \frac{dq}{\sqrt{1-q^2}}$ et integratione $\int \frac{dy}{\dot{Y} + \int \dot{Y} + \ddot{Y} dy + A} + C = \int \frac{dq}{\sqrt{1-q^2}}$. Poſita $\frac{dy}{Y + \int \dot{Y} + \ddot{Y} dy + A} + C = l$ et N baſi logarithmica erit $q = \frac{N^{l\sqrt{-1}} - N^{-l\sqrt{-1}}}{2\sqrt{-1}}$ et $\sqrt{1-q^2} =$

$= \frac{N^{l\sqrt{-1}} + N^{-l\sqrt{-1}}}{2}$ quæ functiones sunt quantitatis y, quibus positis $\overset{\text{IV}}{Y}$ et $\sqrt{1-\overset{\text{II}}{Y}{}^2}$ prodit $x\left(=\int \frac{q dy}{\sqrt{1-q^2}}\right) = \int \frac{\overset{\text{III}}{Y} dy}{\sqrt{1-\overset{\text{II}}{Y}{}^2}}$ æquatio quæ indolem curvarum indicat.

Si $G=X$ functioni ipsius x erit per Cor. 2. $\frac{dx}{X}\left(=\frac{dx}{G}\right) = \frac{qdq}{1-q^2}$, et integratione log. $CN^l\left(=\int\frac{dx}{X} + \log. C\right) = \log. \frac{1}{\sqrt{1-q^2}}$ si $\int\frac{dx}{X} = l$, exinde $\sqrt{1-q^2} = \frac{1}{CN^l}$, $q = \frac{\sqrt{C^2N^{2l}-1}}{CN^l}$ et $y\left(=\int \frac{dx\sqrt{1-q^2}}{q}\right) = \int \frac{dx}{\sqrt{C^2N^{2l}-1}}$, quæ curvæ naturam indigitat.

Si $G=Z$ functioni ipsius z erit $\frac{dz}{Z}\left(=\frac{dz}{G}\right) = \frac{dq}{1-q^2}$, et integratione log. $CN^l\left(=\frac{dz}{Z} + C\right) = \log. \sqrt{\frac{1+q}{1-q}}$ si $\int\frac{dz}{Z} = l$, unde $q = \frac{C^2N^{2l}}{1+C^2N^{2l}}\sqrt{1-q^2} = \frac{2CN^l}{1+C^2N^{2l}}$ $x\left(=\int q dz\right) = \int \frac{\sqrt{C^2N^{2l}-1}dz}{1+C^2N^{2l}}$ et $y\left(=\int dz\sqrt{1-q^2}\right) = \int \frac{2CN^l dz}{1+C^2N^{2l}}$ quibus curvæ cognoscuntur.

Patet hinc quod quando $Y + \int \overset{\text{I}}{Y} dy$ algebraice habeatur $\int \frac{dy}{Y + \int \overset{\text{I}}{Y} + \overset{\text{II}}{Y} dy + A}$ per quadraturam circuli, et $\int \frac{\overset{\text{III}}{Y} dy}{\sqrt{1-\overset{\text{II}}{Y}{}^2}}$ etiam obtineatur algebraice, curvæ evadunt algebraicæ, secus vero transcendentes.

Quando $\int \frac{dx}{X}$ vel $\int \frac{dz}{Z}$ obtineatur per logarithmos, et $\int \frac{dx}{\sqrt{C^2N^{2l}-1}}$, vel tam $\int \frac{\sqrt{C^2N^{2l}-1}dz}{1+C^2N^{2l}}$ quam $\int \frac{2CN^l dz}{1+C^2N^{2l}}$ absoluta integratione, curvæ erunt algebraicæ.

Exempl.

Exempl. 1. Sit index variationis curvaturæ $T=\frac{6y}{a}$ erit $\int T dy = \frac{3y^2}{a}+A$, si quantitas illa constans $A=\frac{a}{2}$ quod evenit quum $\int T dy = \frac{a}{2}$ et $y=0$; sumatur $x=\frac{y^2}{a}$ erit vi theorematis $\frac{2a dy}{a^2+4y^2}$ $\left(=\frac{dy}{\int T dy - x}\right) = \frac{dq}{\sqrt{1-q^2}}$ et integratione $\int \frac{2a dy}{a^2+4y^2}+C=\int \frac{dq}{\sqrt{1-q^2}}$, cujus æquationis termini quoniam sint arcus circulares quorum sinus $q=\frac{2y}{\sqrt{a^2+4y^2}}$ et cosinus $\sqrt{1-q^2}=\frac{a}{\sqrt{a^2+4y^2}}$, arcu constanti $C=0$, obtinetur $x\left(=\int \frac{q dy}{\sqrt{1-q^2}}\right)=\frac{y^2}{a}$ æquatio pro parabola Apolloniana.

Exempl. 2. Si sit $T=\frac{a^2}{y\sqrt{a^2-y^2}}$ habetur $\int T dy = \int \frac{dy\sqrt{a^2-y^2}}{y} - \sqrt{a^2-y^2}+A$, si quantitas illa constans $A=0$ quod evenit quum $\int T dy = 0$ et $y=a$, et assumatur $x=\int \frac{dy\sqrt{a^2-y^2}}{y}$, evadit per theorema $-\frac{dy}{\sqrt{a^2-y^2}}\left(=\frac{dy}{\int T dy - x}\right)=\frac{dq}{\sqrt{1-q^2}}$, et per integrationem $-\int \frac{dy}{\sqrt{a^2-y^2}}+C=\int \frac{dq}{\sqrt{1-q^2}}$, quorum arcuum sinus $q=\frac{\sqrt{a^2-y^2}}{a}$ et cosinus $\sqrt{1-q^2}=\frac{y}{a}$ si constans ille $C=0$, atque inde $dx\left(\frac{q\,dy}{\sqrt{1-q^2}}\right)=\frac{dy\sqrt{1-y^2}}{y}$ qua patet curvam esse tractoriam.

THEOREMA IV.

Dicatur summa tangentium angulorum HCD et BCD H, et differentia tangentium angulorum HCD et CKB K, retentis reliquis denominationibus erit $\frac{dx}{\int H dx}=-\frac{dp}{\sqrt{1-p^2}}$ et $\frac{dy}{\int K dy}=\frac{dq}{\sqrt{1-q^2}}$.

Quoniam

Quoniam $dy = \frac{p\,dx}{\sqrt{1-p^2}}$ erit $dy + T\,dx = \overline{T + \frac{p}{\sqrt{1-p^2}}}\,dx$ et quum $H = T + \frac{p}{\sqrt{1-p^2}}$ habetur $dy + T\,dx = H\,dx$. Eodem modo quum $dx = \frac{q\,dy}{\sqrt{1-q^2}}$ erit $\int T\,dy - dx = T - \frac{q}{\sqrt{1-q^2}}\,dy$, ſed $K = T - \frac{q}{\sqrt{1-q^2}}$, unde $\int T\,dy - x = K\,dy$. Per theorema igitur 2 et 3 provenit $\frac{dx}{\int H\,dx} = -\frac{dp}{\sqrt{1-p^2}}$ et $\frac{dy}{\int K\,dy} = \frac{dq}{\sqrt{1-q^2}}$.

Cor. Si ſit ut antea tangens anguli BCD r, cotangens t, ſecans s, et coſecans v, erit $\frac{dx}{\int H\,dx} = -\frac{dr}{r+r^3}$ et $\frac{dx}{\int H\,dx} = -\frac{ds}{s\sqrt{s^2-1}}$, $\frac{dy}{\int K\,dy} = \frac{dt}{1+t^2}$ et $\frac{dy}{\int K\,dy} = \frac{dv}{v\sqrt{v^2-1}}$.

Schol. Ope hujus theorematis invenire licet curvas, data relatione inter H et x atque K et y. Itaque ſit $H = X$ functioni ipſius x erit $\int H\,dx = \int X\,dx + A$, vi theorematis $\frac{dx}{\int X\,dx + A} \left(= \frac{dx}{\int H\,dx} \right) = -\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int \frac{dx}{\int X\,dx + A} + C = -\int \frac{dp}{\sqrt{1-p^2}}$. Poſita $\int \frac{dx}{\int X\,dx + A} + C = m$, et N logarithmorum baſi prodit $\sqrt{1-p^2} = \frac{N^{m\sqrt{-1}} - N^{-m\sqrt{-1}}}{2\sqrt{-1}}$ et $p = \frac{N^{m\sqrt{-1}} + N^{-m\sqrt{-1}}}{2}$, quibus functionibus quantitatis x poſitis $\sqrt{1-X^2}$ et X provenit æquatio $y \left(= \int \frac{p\,dx}{\sqrt{1-p^2}} \right) = \frac{X\,dx}{\sqrt{1-X^2}}$ naturam curvarum exprimens.

Si $K = Y$ functioni quantitatis y, eadem calculandi ratione habetur $x \left(= \int \frac{q\,dy}{\sqrt{1-q^2}} \right) = \frac{Y\,dy}{\sqrt{1-Y^2}}$ æquatio qua curvæ cognoſcuntur.

Quando

Quando $\int X dx$ vel $\int Y dy$ absoluta integratione, $\int \frac{dx}{\int X dx + A}$ vel $\int \frac{dy}{\int Y dy + A}$ per rectificationem circuli, et $\int \frac{\dot{X} dx}{\sqrt{1-\dot{X}^2}}$ vel $\int \frac{\dot{Y} dy}{\sqrt{1-\dot{Y}^2}}$ integratione perfecta obtineantur, curva est algebraica.

Exempl. 1. Si sit $H = \frac{a+12x}{2\sqrt{a}\sqrt{x}}$ erit $\int H dx = \frac{a+4x\sqrt{x}}{\sqrt{a}} + A$, et posita $A = 0$ habetur per theorema $\frac{dx\sqrt{a}}{a+4x\sqrt{x}} \left(= \frac{dx}{\int H dx}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et per integrationem $\int \frac{dx\sqrt{a}}{a+4x\sqrt{x}} + C = \int \frac{dp}{\sqrt{1-p^2}}$, cujus termini quum sint arcus circulares quorum sinus $\sqrt{1-p^2} = \frac{2\sqrt{x}}{\sqrt{a+4x}}$ et cosinus $p = \frac{\sqrt{a}}{\sqrt{a+4x}}$, posita $C = 0$, obtinetur $y \left(= \int \frac{b dx}{\sqrt{1-p^2}}\right) = \sqrt{ax}$, quæ parabolam Apolloniam exprimit.

Exempl. 2. Sit $H = \frac{2a^4 - x^4}{ax^2\sqrt{a^4 - x^4}}$ erit $\int H dx = \frac{x^2 - 2a^2\sqrt{a^2 - x^2}}{ax} + A$, et si $A = 0$, per theorema $\frac{axdx}{x^2 + 2a^2\sqrt{a^2 - x^2}} \left(= \frac{dx}{\int H dx}\right) = -\frac{dp}{\sqrt{1-p^2}}$ et per integrationem $\int \frac{axdx}{x^2 - 2a^2\sqrt{a^2 - x^2}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, et si $C = 0$, $\sqrt{1-p^2} = \frac{\sqrt{a^2 - x^2}}{\sqrt{2a^2 - x^2}}$ $p = \frac{a}{\sqrt{2a^2 - x^2}}$ et $y \left(= \int \frac{p dx}{\sqrt{1-p^2}}\right) = \int \frac{adx}{\sqrt{a^2 - x^2}}$, æquatio pro curva sinuum.

Exempl. 3. Si sit $K = \frac{5a^2 + 6y^2 \cdot y}{a\sqrt{a^2 + y^2}}$ erit $\int K dy = \frac{a^2 + 2y^2\sqrt{a^2 + y^2}}{a^2} + A$, si $A = 0$ habetur per theorema $\frac{a^2 dy}{a^2 + 2y^2\sqrt{a^2 + y^2}} \left(= \frac{dy}{\int K dy}\right) = \frac{dq}{\sqrt{1-q^2}}$ et integratione $\int \frac{a^2 dy}{a^2 + 2y^2\sqrt{a^2 + y^2}} + C = -\int \frac{dp}{\sqrt{1-p^2}}$, qua $q = \frac{y}{\sqrt{a^2 + 2y^2}}$, $\sqrt{1-q^2} = \frac{\sqrt{a^2 + y^2}}{\sqrt{a^2 + 2y^2}}$, si $C = 0$, unde $x \left(= \int \frac{q dy}{\sqrt{1-q^2}}\right) = \sqrt{a^2 + y^2}$ æquatio pro hyperbola æquilatera.

Exempl.

Exempl. 4. Sit $K = \frac{y}{\sqrt{a^2-y^2}}$ erit $\int K dy = A - \sqrt{a^2-y^2}$ et si $A = 0$, per theorema $-\frac{dy}{\sqrt{a^2-y^2}} \left(= \frac{dy}{\int K dy}\right) = \frac{dq}{\sqrt{1-q^2}}$ et per integrationem $-\int \frac{dy}{\sqrt{a^2-y^2}} + C = \int \frac{dq}{\sqrt{1-q^2}}$ qua $q = \frac{\sqrt{a^2-y^2}}{a}$, $\sqrt{1-q^2} = \frac{y}{a}$ et $dx \left(= \frac{q dy}{\sqrt{1-q^2}}\right) = \frac{dy\sqrt{a^2-y^2}}{y}$ quæ Tractoriam exprimit.

THEOREMA V.

Designetur productum tangentium angulorum HCD et BCD per U, et angulorum HCD et CKB per V cæteris manentibus erit $\frac{dx}{\int U dx - x} = -\frac{dp}{p}$ et $\frac{dy}{Y + \int V dy} = \frac{dq}{q}$.

Quoniam $dy = \frac{p dx}{\sqrt{1-p^2}}$ et $U = \frac{Tp}{\sqrt{1-p^2}}$ erit $T dy \left(= \frac{Tp dx}{\sqrt{1-p^2}}\right) = U dx$, et integratione $\int T dy = \int U dx$ qua $\int T dy - x = \int U dx - x$. Et quoniam $dx = \frac{q dy}{\sqrt{1-q^2}}$ et $V = \frac{Tq}{\sqrt{1-q^2}}$ erit $T dx \left(= \frac{Tq dy}{\sqrt{1-q^2}}\right) = V dy$, $\int T dx = \int V dy$ et $y + \int T dx = y + \int V dy$. Theoremate 2. et 3. prodit $\frac{dx}{\int U dx - x} = -\frac{dp}{p}$ et $\frac{dy}{y + \int V dy} = \frac{dq}{q}$.

Cor. Si anguli BCD tangens, cotangens, &c. designentur ut antea, habetur $\frac{dx}{\int U dx - x} = -\frac{dr}{r \cdot 1 + r^2}$, $\frac{dy}{y + \int V dy} = -\frac{dt}{t \cdot 1 + t^2}$, &c.

Schol. Per hoc theorema curvæ inveniuntur ex data relatione inter U et x, atque inter V et y. Si enim sit $U = X$ functioni ipsius x erit $\int U dx = \int X dx + A$, per theorema $\frac{dx}{\int X dx - x + A} \left(= \frac{dx}{\int U dx - x}\right) = -\frac{dp}{p}$, et per integrationem $\int \frac{dx}{\int X dx - x + A} + \log. C =$

log. $\frac{1}{p}$. Ponatur $\int \frac{dx}{\int Xdx - x + A} = n$ et N basi logarithmica, erit $\frac{1}{p} = CN^n$, $p = \frac{1}{CN^n}$, $\sqrt{1-p^2} = \frac{\sqrt{C^2N^{2n}-1}}{CN^n}$ et $y \left(= \frac{pdx}{\sqrt{1-p^2}}\right) = \int \frac{dx}{\sqrt{C^2N^{2n}-1}}$ qua æquatione curvarum indoles innotescit.

Si V = Y functioni ipsius y, eadem calculandi ratione proveni t $x \left(= \int \frac{qdy}{\sqrt{1-q^2}}\right) = \int \frac{CN^n dy}{\sqrt{1-C^2N^{2n}}}$ qua curvæ cognoscuntur.

Evidens hinc est quod quoties $\int Xdx$ vel $\int Ydy$ algebraice $\int \frac{dx}{\int Xdx - x + A}$ vel $\int \frac{dy}{y + \int Ydy + A}$ per logarithmos, atque $\int \frac{dx}{\sqrt{C^2N^{2n}-1}}$ vel $\int \frac{CN^n dy}{\sqrt{1-C^2N^{2n}}}$ integratione absoluta, obtineantur, curva est algebraica.

Exempl. 1. Si sit U = 3 erit $\int Udx = 3x + A$, si vero $\int Udx = \frac{a}{2}$ quando $x = 0$ erit $A = \frac{a}{2}$ et $\int Udx - x = \frac{a+4x}{2}$. Per theorema igitur $\frac{2dx}{a+4x} \left(= \frac{dx}{\int Udx - x}\right) = -\frac{dp}{p}$ et per integrationem log. $\sqrt{a+4x}$ + log. C = log. $\frac{1}{p}$, posita $p = 1$ dum $x = 0$ log. C = − log. $\sqrt{a}$, unde facto a logarithmis transitu $\frac{\sqrt{a+4x}}{\sqrt{a}} = \frac{1}{p}$, qua $p = \frac{\sqrt{a}}{\sqrt{a+4x}}$, $\sqrt{1-p^2} = \frac{2\sqrt{x}}{\sqrt{a+4x}}$ et $y \left(= \int \frac{pdx}{\sqrt{1-p^2}}\right) = \int \frac{dx\sqrt{a}}{2\sqrt{x}}\right) = \sqrt{ax}$ æquatio pro Parabola Apolloniana.

Exempl. 2. Sit $U = \frac{x^3 - 4a^3}{x\sqrt{x}}$ erit $\int Udx = \frac{x^3 - 2a^3}{3x^2} + A$, si autem $\int Udx = 0$ et $x = a\sqrt[3]{2}$, erit $A = 0$ et $\int Udx - x = \frac{2 \cdot a^3 - x^3}{3x^2}$. Vi igitur theorematis erit $\frac{3x^2 dx}{2a^3 - x^3} \left(= \frac{dx}{\int Udx - x}\right) = -\frac{dp}{p}$, et integratione

$\log. \frac{a\sqrt{a}}{\sqrt{a^3-x^3}} + \log. C = \log. \frac{1}{p}$ qua $p = \frac{\sqrt{a^3-x^3}}{a\sqrt{a}}$; $\sqrt{1-p^2} = \frac{x\sqrt{x}}{a\sqrt{a}}$ et $y \left(= \int \frac{pdx}{\sqrt{1-p^2}}\right) = \frac{dx\sqrt{a^3-x^3}}{x\sqrt{x}}$ æquatio ad curvam quæsitam.

Exempl. 3. Si $V = -\frac{1}{2}$ erit $\int V dy = A - \frac{y}{2}$, posita $\int V dy = o$ et $y = o$ erit $A = o$ et $y + \int V dy = \frac{y}{2}$. Per theorema obtinetur $\frac{2dy}{y} \left(= \frac{dy}{y + \int V dy}\right) = \frac{dq}{q}$ et per integrationem $\log. y^2 + \log. C = \log. q$, si $q = 1$ et $y = a$ erit $\log. C = -\log. a^2$, unde $q = \frac{y^2}{a^2}$, $\sqrt{1-q^2} = \frac{\sqrt{a^4-y^4}}{a^2}$ atque $dx \left(= \frac{qdy}{\sqrt{1-q^2}}\right) = \frac{y^2 dy}{\sqrt{a^4-y^4}}$, curva ergo est Elastica,

Exempl. 4. Sit $V = \frac{a^2-2y^2}{y^2}$ erit $\int V dy = A - \frac{a^2+2y^2}{y}$, si $\int V dy = -3a$ et $y = a$ erit $A = o$, indeque $y + \int V dy = -\frac{a^2+y^2}{y}$. Theorematis ope habetur $-\frac{ydy}{a^2+y^2} \left(= \frac{dy}{y + \int V dy}\right) = \frac{dq}{q}$ et integratione $\log. \frac{1}{\sqrt{a^2+y^2}} + \log. C = \log. q$, si $q = 1$ et $y = o$ erit $\log. C = \log. a$ et exinde $q = \frac{a}{\sqrt{a^2+y^2}}$, $\sqrt{1-q^2} = \frac{y}{\sqrt{a^2+y^2}}$ et $dx \left(= \frac{qdy}{\sqrt{1-q^2}}\right) = \frac{ady}{y}$ æquatio pro Logarithmica.

THEOREMA VI.

Dicatur ED L, et AE M, retentis præterea adhibitis denominationibus erit $\frac{dL}{T} = dx$ et $\frac{dM}{T} = dy$.

Quoniam $dx : dx :: T dz \ (dR) : T dx$ habetur $dL = T dx$ et

$\frac{dL}{T} = dx$. Et quoniam $dx : dy :: T dx (dR) : T dy$ obtinetur $dM = T dy$ et $\frac{dM}{T} = dy$.

Cor. Quum $T dy = U dx$ et $T dx = V dy$, erit substitutione $\frac{dM}{U} = dx$ et $\frac{dL}{V} = dy$.

Schol. Hoc adhibito theoremate inveniri possunt curvæ data relatione inter T et L, T et M, atque inter U et M et V et L. Ponatur $L = \overset{1}{T}$ functioni quantitatis T habetur per theorema $\frac{d\overset{1}{T}}{T} \left(= \frac{dL}{T}\right) = dx$ et integratione $\int \frac{d\overset{1}{T}}{T} + C = x$ qua T per x datur. Curvæ deinde per theorema 2. elici possunt.

Si $M = \overset{1}{T}$ ipsius T functioni, habetur eodem modo T per y. Si $M = \overset{1}{U}$ functioni ipsius U, obtinetur U per x, et si $L = \overset{1}{V}$ functioni quantitatis V, datur V per y. Per theorema deinde 3. et 5. curvæ inveniuntur.

Evidens quidem est quod curvæ esse non possunt algebraicæ nisi $\int \frac{dL}{T}$, $\int \frac{dM}{T}$, $\int \frac{dM}{U}$ vel $\int \frac{dL}{V}$, obtineantur integratione absoluta.

Exempl. 1. Si sit $L = \frac{a T^3}{54}$ erit $dL = \frac{a T^2 dT}{18}$, et per hoc theorema $\frac{a T dT}{18} \left(= \frac{dL}{T}\right) = dx$ et integratione $\frac{a T^2}{36} + C = x$ qua $T = \frac{6\sqrt{x}}{\sqrt{a}}$, si $C = 0$. Per theorema 2. reperitur $= \sqrt{ax}$, æquatio pro Parabola Apolloniana.

Exempl. 2. Si sit $M = -\int \frac{a T^3 dT}{2 \cdot 1 + T^2|^2}$ erit $dM = -\frac{a T^2 dT}{2 \cdot 1 + T|^2}$ et ope theorematis $-\frac{a T dT}{2 \cdot 1 + T^2|^2} \left(= \frac{dM}{T}\right) = dy$, et integratione $\frac{a}{4 \cdot 1 + T^2} + C = y$, qua si $C = 0$, $T = \frac{\sqrt{a - 4y}}{2\sqrt{y}}$. Per theorema 3. habetur

$dx =$

$dx = \frac{2dy\sqrt{y}}{\sqrt{a-4y}}$, æquatio pro Cycloide ordinaria.

Exempl. 3. Sit $L = -a\sqrt{V}$ erit $dL = -\frac{adV}{2\sqrt{V}}$ et per theorema $-\frac{adV}{2V\sqrt{V}}\left(=\frac{dL}{V}\right) = dy$ et integratione $\frac{a}{\sqrt{V}} + C = y$, et si $C = o$, habetur $V = \frac{a^2}{y^2}$ et deinde per theorema 5. $dx = \frac{dy\sqrt{a^2-y^2}}{y}$, qua constat curvam esse Tractoriam.

THEOREMA VII.

Dicatur ut antea CF F et CG G, et summa tangentium angulorum HCD et BCD, H, et differentia tangentium angulorum HCD et CKB, K, erit $\frac{dF}{H} = dx$ et $\frac{dG}{K} = dy$.

Quoniam $dF\ (= dy + Tdx) = Hdx$ et $dG\ (= \int T dy - x) = Kdy$ provenit $\frac{dF}{H} = dx$ et $\frac{dG}{K} = dy$.

Cor. Quum $F = -\frac{dx\sqrt{1-p^2}}{dp}$ et $G = \frac{dy\sqrt{1-q^2}}{dp}$ provenit divisione $\frac{dF}{FH} = -\frac{dp}{\sqrt{1-p^2}}$ atque $\frac{dG}{GK} = \frac{dq}{\sqrt{1-q^2}}$.

Schol. Auxilio hujus theorematis inveniuntur curvæ ex data relatione inter F et H, G et K, H et p atque K et q. Nam si sit $F = \overset{1}{H}$ functioni ipsius H, vel $G = \overset{1}{K}$ functioni ipsius K, habetur per theorema $\frac{d\overset{1}{H}}{H}\left(=\frac{dF}{H}\right) = dx$ et integratione $\int\frac{d\overset{1}{H}}{H} + C = x$ qua H per x datur. Eodem modo $\frac{d\overset{1}{K}}{K}\left(=\frac{dG}{K}\right) = dy$ et integratione $\int\frac{d\overset{1}{K}}{K} + C = y$ qua K per y obtinetur. Theorema 4. ulterius progredienti viam monstrat ad curvas inveniendas.

Patet

Patet quod curva non ſit algebraica niſi $\int \frac{dH}{H}$ vel $\int \frac{dK}{K}$ obtineantur perfecta integratione.

Exempl. 1. Si ſit $F = \frac{a}{\sqrt{1+H^2}}$ habetur per theorema $-\frac{adH}{\overline{1-H^2}^{\frac{3}{2}}}\left(=\frac{dF}{H}\right) = dx$, et integratione $\frac{aH}{\sqrt{1-H^2}} + C = -x$ qua $H = -\frac{x}{\sqrt{a^2-x^2}}$, poſita $C = 0$. Per theorema deinde 4. provenit $y = \sqrt{a^2-x^2}$ æquatio pro circulo.

Exempl. 2. Sit $F = \frac{a \cdot H^3 + H^3 + 6\sqrt{H^2-12}}{108}$, erit per theorema $\frac{a \cdot H^2 - 6 + H\sqrt{H^2-12} \cdot dH}{36\sqrt{H^2-12}}\left(= \frac{dF}{H}\right) = dx$ et integratione facta $\frac{a \cdot H^2 - 6 + H\sqrt{H^2-12}}{72} + C = x$, et poſita $C = 0$ habetur $H = \frac{a+12x}{2\sqrt{a}\sqrt{x}}$, unde per theorema 4. prodit $y = \sqrt{ax}$ æquatio pro Parabola Apolloniana.

Exempl. 3. Sit $G = -\frac{a \cdot \sqrt{4+K^2}}{4}$ erit per theorema $\frac{adK}{2}\left(= \frac{dG}{K}\right) = dy$, et integratione $\frac{aK}{2} + C = y$, et ſi $C = 0$ $K = \frac{2y}{a}$ unde per theorema 4. $dx = \frac{ady}{y}$, qua conſtat curvam eſſe Logarithmicam.

THEOREMA VIII.

Dicatur ut antea productum tangentium angulorum HCD et BCD U, et productum tangentium angulorum HCD et CKB V manentibus reliquis denominationibus erit $\frac{dG}{U-1} = dx$ et $\frac{dF}{1+V} = dy$.

Quoniam

Quoniam $G = \int T dy - x$ erit $dG = T dy - dx$, ſed $T dy = U dx$, unde $dG = \overline{U-1}\,dx$ et $\frac{dG}{U-1} = dx$. Eodem modo quum $F = y + \int T dx$ erit $dF = dy + T dx$, ſed $T dx = V dy$ quare $dF = \overline{1+V}\,dy$ et $\frac{dF}{1+V} = dy$.

Cor. Quoniam $G = \frac{dy\sqrt{1-q^2}}{dq}$ et $F = -\frac{dx\sqrt{1-p^2}}{dp}$, habetur ſubſtitutione debita $\frac{dG}{G \cdot U - 1} = -\frac{dp}{p}$ et $\frac{dF}{F\sqrt{1+V}} = \frac{dq}{q}$.

Schol. Ope hujus theorematis indagantur curvæ data relatione inter G et U vel inter F et V. Nam ſi ſit $G = \dot{U}$ functioni quantitatis U vel $F = \dot{V}$ functioni quantitatis V obtinetur per theorema in caſu priori $\frac{d\dot{U}}{\overline{U-1}}$ $\left(= \frac{dG}{\overline{U-1}}\right) = dx$ et integratione $\int \frac{d\dot{U}}{\overline{U-1}} + C = x$, qua U per x habetur; in poſteriori $\frac{d\dot{V}}{\overline{1+V}}$ $\left(= \frac{dF}{\overline{1+V}}\right)$ $= dy$ et integratione $\int \frac{d\dot{V}}{\overline{1+V}} + C = y$, qua V habetur per y. Per theorema deinde 5. curvæ cognoſcuntur.

Datur etiam per Cor. U in p, et V in q, et conſequenter T in p vel q, nam $U = \frac{Tp}{\sqrt{1-p^2}}$ et $V = \frac{Tq}{\sqrt{1-q^2}}$.

Conſtat hinc quod curvæ non ſint algebraicæ niſi $\int \frac{d\dot{U}}{\overline{U-1}}$ vel $\int \frac{d\dot{V}}{\overline{1+V}}$ obtineantur integratione abſoluta.

Exempl. 1. Si ſit $G = \frac{a \cdot \overline{U-3}}{2}$ erit per theorema $\frac{adU}{2\overline{U-1}}$ $\left(= \frac{dG}{\overline{U-1}}\right) = dx$ et integratione $\log. 1 - U + \log. C = \frac{2x}{a}$ et ſi $C = \frac{a^2}{2}$

log.

log. $\frac{a^2 \cdot \overline{1-U}}{2} = \frac{2x}{a}$ et $\frac{a \cdot \overline{1-U}}{2} = N^{\frac{2x}{a}}$ qua $U = \frac{a^2 - 2N^{\frac{2x}{a}}}{a^2}$. Per theorema deinde 5. habetur $dy = \frac{dx N^{\frac{x}{a}}}{a}$ qua conſtat curvam eſt Logarithmicam.

Exempl. 2. Si ſit $T = \frac{a \cdot \overline{V-1}\sqrt{V+2}}{3\sqrt{3}}$ erit per theorema $\frac{adV}{2\sqrt{3}\sqrt{V+2}} \left(= \frac{dF}{1+V}\right) = dy$ et per integrationem $\frac{a\sqrt{V+2}}{\sqrt{3}} = y$ qua $V = \frac{3y^2 - 2a^2}{a^2}$; et per theorema 5. $dx = \frac{dy\sqrt{y^2-a^2}}{a}$, æquatio ad curvam cujus conſtructio a quadratura hyperbolæ dependet.

THEOREMA IX.

Sint LC et *l c* duæ curvæ eandem habentes Evolutam QD, dicatur radiorum oſculi CD *c*D conſtans differentia *c*C *b*, curvæ *l c* variatio curvaturæ S, ceteriſque ut antea manentibus erit $\frac{dR}{R-bS} = -\frac{dp}{\sqrt{1-p^2}}$.

Quoniam radius curvaturæ DH evolutæ ſit $RT = R - bS$, erit $\frac{1}{R-bS} = \frac{1}{RT}$, quæ per $dR\ (= Tdz) = -\frac{RTdp}{\sqrt{1-p^2}}$ multiplicata, moſtrat eſſe $\frac{dR}{R-bS} = -\frac{dp}{\sqrt{1-p^2}}$.

Cor. Si ſint ut antea tangens anguli BCD r et ſecans s, habetur $\frac{dR}{R-bS} = -\frac{dr}{1+r^2}$ et $\frac{dR}{R-bS} = -\frac{ds}{S\sqrt{S^2-1}}$.

Schol. Subſidio hujus theorematis invenire licet curvas, data relatione inter S et R vel inter S et T nam $\frac{S}{T} = \frac{R}{R-b}$. Itaque ſi ponatur

ponatur $S = R$ functioni radii curvedinis R, erit $\frac{dR}{R-bR}\left(=\frac{dR}{R-bS}\right)$ $= -\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int\frac{dR}{R-bR}+C=-\int\frac{dp}{\sqrt{1-p^2}}$. Sit $\int\frac{dR}{R-bR}+C=f$ et N logarithmorum basi habetur $\sqrt{1-p^2}=\frac{N^{f\sqrt{-1}}-N^{-f\sqrt{-1}}}{2\sqrt{-1}}$ et $p=\frac{N^{f\sqrt{-1}}+N^{-f\sqrt{-1}}}{2}$ functionibus quantitatis R, quibus R per p exprimi potest. Per theorema igitur 1. curvas internoscere valemus.

Si $R=S$ functioni quantitatis S habetur $\frac{dS}{S-bS}\left(=\frac{dR}{R-bS}\right)$ $= -\frac{dp}{\sqrt{1-p^2}}$, et integratione $\int\frac{dS}{S-bS}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, posita $\int\frac{dS}{S-bS}+C=g$, erit $\sqrt{1-p^2}=\frac{N^{g\sqrt{-1}}-N^{-g\sqrt{-1}}}{2\sqrt{-1}}$ et $p=\frac{N^{g\sqrt{-1}}+N^{-g\sqrt{-1}}}{2}$ quibus S per p datur. Per theoremata Partis I. invenire licet curvas omnes eandem evolutam habentes.

Hinc videtur, quod curvæ non sint algebraicæ nisi $\int\frac{dR}{R-bR}$ vel $\int\frac{dS}{S-bS}$ per circuli rectificationem obtineatur.

Exempl. 1. Si sit $S=\frac{2R}{\sqrt{a}.\sqrt{R-a}}$ suppositâ $b=a$, erit per theorema $\frac{dR\sqrt{a}}{2R\sqrt{R-a}}\left(=\frac{dR}{R-bS}\right)=-\frac{dp}{\sqrt{1-p^2}}$ et integratione $\int\frac{dR\sqrt{a}}{2R\sqrt{R-a}}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, si vero arcus ille constans $C=0$ erit $\sqrt{1-p^2}=\frac{\sqrt{R-a}}{\sqrt{R}}$ qua $R=ap^2$, et per Cor. 1. Theor. 1. habetur $dy=\frac{adx}{\sqrt{x-a}}$, æquatio pro Catenaria.

Exempl. 2. Sit $S=\frac{5a^2+R^2}{a\cdot a-5R}$, posita $b=\frac{a}{5}$, erit per theorema $-\frac{adR}{a^2+R^2}\left(=\frac{dR}{R-bS}\right)=-\frac{dp}{\sqrt{1-p^2}}$ et facta integratione $-\int\frac{adR}{a^2+R^2}+C=-\int\frac{dp}{\sqrt{1-p^2}}$, qua si $C=0$, habetur $\sqrt{1-p^2}=\frac{R}{\sqrt{a^2+R^2}}$ et $R=\frac{a\sqrt{1-p^2}}{p}$. Per theorema 1. $dx=\frac{dy\sqrt{a^2-y^2}}{y}$ qua constat curvam esse Tractoriam.

Fig. 1.

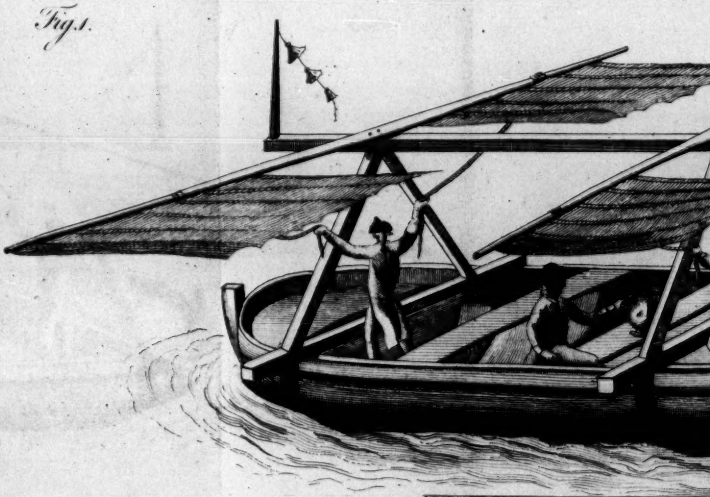
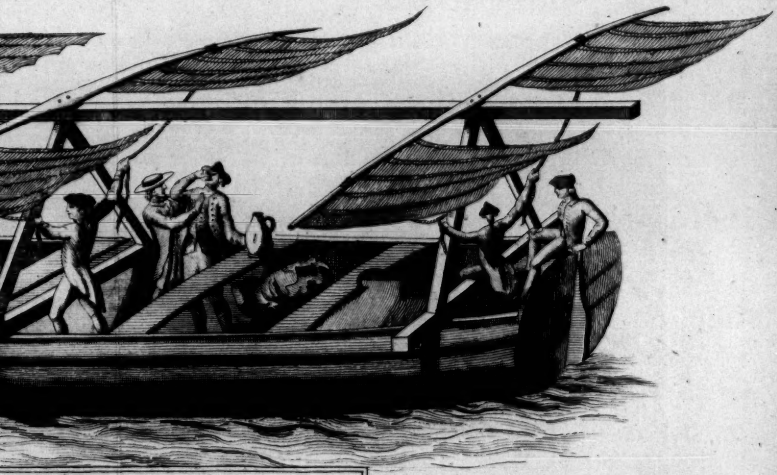
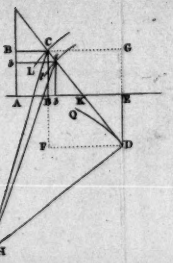


Fig. 2.

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290. 11. *dele* rather

291. 8. *for* the *read* theſe

329. 7. *for* 1784 *read* 1783

341. 23. *for* , firſt, *read* fixed

342. 12. *for* than *read* that

345. 1. *after* from *inſert* their

365. 11. *for* I *read* K

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In plate XXI. fig. 2. at the lower angle of the diagram, in ſome copies, the letter K ſtands by miſtake inſtead of H.